Ronald W. Casson

Language, Culture, and Cognition

ANTHROPOLOGICAL PERSPECTIVES

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State Institute of Education
P.O. Banipur. 24 Parganas.
West Bengal.

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Ronald W. Casson *Oberlin College*

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Preface

For the last several years, in organizing reading materials for my course in cognitive anthropology, I have found that I have increasingly had to rely on xerox copies of articles that can be put on reserve at the library. No new collection of articles in this field of cultural anthropology has appeared since Stephen A. Tyler's *Cognitive Anthropology* in 1969 and James P. Spradley's *Culture and Cognition: Rules, Maps, and Plans* in 1972. These anthologies, both excellent collections of work published mostly in the 1960s, have become somewhat dated and are no longer available for course adoption. Furthermore, no general textbook covering cognitive anthropology has been published. Robbins Burling's *Man's Many Voices: Language in Its Cultural Context* is still the only textbook containing material that can be used to introduce students to the field. One of the aims of this book is to meet this need for up-to-date introductory material.

Other aims of the book are to provide a survey of current issues and research in cognitive anthropology and to characterize the theoretical paradigm that presently guides research in this field. This paradigm has changed considerably during the past decade, but there has been no recent attempt to assess these changes or represent the present nature and scope of the field. After the first section, in which the principal topics—language, culture, and cognition—are introduced and their interrelationships are described, the four subsequent sections focus on the major areas of current research in cognitive anthropology and recent developments in these areas; the second section is concerned with folk classification systems and the relativity/universality issue; the third section deals with the extension and transfer of meaning in classification systems; the fourth section takes up questions of variability and change in classification systems; and the fifth section examines the interrelationships between cognitive systems and actual behaviors and contexts. The articles in these sections were selected to emphasize contemporary perspectives and directions of research. They were all originally published in the 1970s.

The book begins with a General Introduction, which describes the history and development of cognitive anthropology, the basic theoretical orientation and principal research interests of cognitive anthropologists, and the relationship of cognitive anthropology to other subdisciplines within cultural anthropology.

Each of the five sections of the book also begins with an introduction, which provides background information and summary explanations of the articles in the section. The contribution of each article to cognitive anthropology is discussed, and the interconnections among the articles comprising the section are examined in order to characterize larger patterns. The aim is to elucidate the nature and scope of the major research areas in cognitive anthropology and, thereby, to represent the shape of the field as a whole. It is my hope that this overall representation of the nature and scope of cognitive anthropology accurately depicts the current "state of the art."

I have received a great deal of assistance in preparing this book. The authors of the articles reprinted here have been exceedingly cooperative and helpful, and many of them have contributed suggestions that have been very useful to me in organizing the book and in writing my introductions. Keith H. Basso and Roy G. D'Andrade provided particularly detailed and valuable comments, many of which have led to significant changes in the contents and organization of the book. Charles O. Frake read the revised version of my article (Chapter 11) and, yet again, offered pertinent advice, for which—as always—I am most grateful. Oberlin students who have been the classroom subjects of my experiments in textbook design and function have—in their customary purposeful manner—informed me of numerous abominable failures and shortcomings, many of which I have hopefully remedied. The editors at Macmillan have also rendered important assistance. I want particularly to express my appreciation to Kenneth J. Scott, Senior Editor, for his guidance and patience, and Ellen Rope and Dave Novack, Production Supervisors, for their thorough and professional handling of the manuscript. I also want to express my gratitude to Sayín Muhterem and Mustafa Özertuğ for their generous hospitality and support, which aided me immeasurably in writing the introductory sections. Most of all I want to thank Bânû Özertuğ-Casson, for everything.

R. W. C.

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Language, Culture, and Cognition: Anthropological Perspectives

General Introduction

WHAT COGNITIVE ANTHROPOLOGISTS STUDY

Perhaps the best means of gaining an initial understanding of the nature and scope of cognitive anthropology—the subfield of cultural anthropology in which the interrelationships among language, culture and cognition are investigated—is to take a look at examples of research on issues that occupy a central place in the field. Cognitive anthropologists have for a long time focused a considerable amount of research time on the words people use for objects, events, and activities that are important in their world. This can be seen in research among *street junkies* (urban heroin addicts) that focuses on the language they use in talking about the most central events in their daily life, *hustling* (getting money), *copping* (buying heroin) and *getting off* (injecting heroin). Michael Agar, the cognitive anthropologist who did this research (Chapter 5), explores getting off in detail and shows that *junkies* conceptualize this activity as a process involving several stages: *cooking* (heating a mixture of heroin and water so as to dissolve the heroin), *drawing* (transferring the mixture into the *works,* the instrument used in injecting the substance), *tying* (using a belt, nylon stocking, or piece of string as a tourniquet to ready a vein for injection), *hitting* (inserting the works into the vein), and *shooting* (forcing the heroin mixture from the works into the vein) or *booting* (injecting a portion of the heroin into the vein, drawing blood and heroin back into the works and then reinjecting the mixture into the vein). In studying the meanings of these words and their significance in this event that is crucial in the life of street junkies, the cognitive anthropologist attempts to comprehend the addict's view of his world.

To take another, somewhat more mundane example, cognitive anthropologists have devoted a great deal of research attention to the words used by different peoples in referring to the plants and animals in their environments. As in the study of heroin addicts, the interest is in examining the language of group members in order to penetrate their world, in this case, in order to understand the way they conceptualize their plant and animal worlds. Brent Berlin, who is one of the leading experts in this area of research, provides a description of the Aguaruna Jivaro version of the plant kingdom (Chapter 4). The Aguaruna, who are native American cultivators of manioc in Peru, classify the plants in their tropical rain forest environment into hierarchically organized categories:

they recognize a category that includes all their plants, they have a set of four major life form categories into which the majority of these plants are classified (categories on the order of English *tree, bush, grass*); they have a very large number of generic categories which comprise the core of their ethnobotanical classification system (categories on the order of English *oak, ivy, bean, bamboo*); and they have relatively fewer categories of specific and varietal plants (categories on the order of English *lima bean* and *baby lima bean,* respectively).

Multiple Meanings

The various types and shadings of meaning that are conveyed and understood by society members are another focus of interest among cognitive anthropologists. To take an especially well-studied example, the word *amma* in Kannada, a Dravidian language spoken in India, has a particularly wide range of interrelated meanings. As shown by Susan S. Bean (Chapter 9), the most basic meaning of *amma* is mother, but it is also used for certain female relatives who are not actual mothers, such as one's mother's elder sister, for any woman who is entitled to respect, for certain malevolent, multi-armed Hindu goddesses, for diseases which cause sores, such as smallpox and chicken pox, and as a distress cry, much like English "Help!"

The problem of understanding figurative meanings in the same way that society members understand them is particularly well illustrated by Western Apache *wise words,* which are a genre of folk sayings, such as "Lightning is a boy" and "Ravens are widows." Keith H. Basso, a cognitive anthropologist who has done extensive research among the Western Apache, quotes the following explanation of "Butterflies are girls" by one of his Apache consultants:

> Let me tell you a story. One time my mother was sick and went to the hospital in Whiteriver. It was before my older sister got married. She was supposed to look after us, cook for us. She did all right, but then one day she took off with one of my [female] cousins and they went to where some people were getting ready for a dance. They stayed there all morning. Then they went to another camp to drink beer with some boys. Then they went to another camp. At night they went back to the dance. Finally, they came home. My grandmother had come to take care of us, and I guess she knew that my sister had been running around. When my sister came in my grandmother didn't say anything at first. Then she said to my older brother, "Butterflies are girls and one of them just flew in." My sister knew what it meant, I guess, because she started feeling bad . . . That's how they use "wise words," these old people—when they want to say something bad about someone. (Chapter 12, pp. 254–255)

The cognitive anthropologist studying Western Apache culture, if he is to understand wise words as a society member understands them, must learn that wise words are used only to express mild criticism (never, for example, to make favorable comparisons of beauty, e.g., girls are as pretty and colorful as butterflies), and that because this criticism is oblique the person or persons at whom they are directed must be inferred from background knowledge and contextual information.

How Context Influences Meaning

Cognitive anthropologists are also concerned with the influence exerted on meaning and behavior by sociocultural context. Charles O. Frake, one of the founders

of cognitive anthropology, discusses exchanges of remarks about mothers (or muh-thuhs) among urban adolescent males in Philadelphia to illustrate the role of context in the interpretation of "what is going on now" (Chapter 17). When the question,

"Hey, where'd you go last night?"

is answered by,

"I was out with yuh muh-thuh, man."

it is interpreted in some contexts as a nonserious or ritual insult, but in others as a real insult. Whether the context is a ritual one in which insult exchanges are to be interpreted as playful verbal dueling or whether it is a serious one in which they are to be interpreted as real conflict or fighting is established on the basis of verbal and nonverbal cues: tone of voice, loudness, facial expression, body posture and movement, and so on.

A detailed example of how context constrains behavior and the interpretation of behavior is recounted by R. P. McDermott and his associates (Chapter 18). The behavior they discuss is a few seconds of interaction among six schoolchildren and their teacher during a reading lesson in a first-grade classroom. Individually and collectively, teacher and children assume the postural configuration appropriate for reading activity: they organize themselves around a reading table, each one focusing on and leaning into his or her book. The positioning of bodies establishes the context; the participants embody the context and fit their behavior to this embodiment. So what is happening—reading—is organized and interpreted on the basis of this mutually constructed context.

What all these examples of research in cognitive anthropology share is a concern with the insider's view, the society member's perspective on what is what, what is the meaning of things, and what is going on now. This has been and remains the principal aim of cognitive anthropology: to understand and describe the world of people in other societies in its own terms, as it is conceived and experienced by the people themselves. The aim is to avoid the biases induced by ethnocentrism—the distortion that results when an investigator imposes his or her outsider's view on life in another society and describes it, usually in negative terms, from this external perspective. A cognitive anthropologist does not, for example, utilize the categories and distinctions of Western biological science in describing another people's version of the plant world, since this would undoubtedly result in a distorted picture of the indigenous system of plant classification.

THE HISTORY OF COGNITIVE ANTHROPOLOGY

The desire to avoid such ethnocentric distortions in describing other societies was a major factor motivating the development of cognitive anthropology. A good deal of the early work in the field—during the late 1950s and early 1960s—emphasized new methods and techniques for doing ethnography (that is, for conducting descriptive studies of other societies and cultures) and one of the first names proposed for the burgeoning new field was the *New Ethnography*. But the New Ethnography was not new in its insistence that the task of the ethnographer is to describe behavior and experience from the perspective of

society members. This is one of the essential messages of cultural anthropology, established as a core component of the anthropological perspective in the early decades of this century. Franz Boas's insistence on the relativity of cultures, on the idea that cultures are unique and must be understood in their own terms apart from the standards and values of Western society, established this notion in American anthropology; Bronislaw Malinowski's argument that extended fieldwork is necessary for a thorough understanding of unfamiliar societies contributed to the establishment of this view in British anthropology.

What was new and crucially important in the New Ethnography was not its emphasis on studying the cultures of other societies from the inside, but rather its position that the object of study is a mental phenomenon. Cognitive anthropology originated in the middle 1950s as the result of a shift in the definition of culture, the single most important concept in cultural anthropology: whereas previously culture was regarded as including behavior and events in the physical world, it was redefined more narrowly at this time as a system of knowledge—as an internal conceptual system underlying and guiding actual behavior and observable events (cf. D'Andrade 1979:1). The date of this shift can be pinpointed as 1957; it was in this year that Ward H. Goodenough published the article containing his highly influential definition of culture as cognition:

> A society's culture consists of whatever it is one has to know or believe in order to operate in a manner acceptable to its members. Culture is not a material phenomenon; it does not consist of things, behavior, or emotions. It is rather an organization of these things. It is the form of things that people have in mind, their models for perceiving, relating, and otherwise interpreting them. (1957:167)

This ideational conception of culture distinguishes cognitive anthropology both from the anthropology of predessors like Boas and Malinowski and from contemporary schools of materialist anthropology, in which culture is treated more broadly as including patterns of behavior and their role in the society's environmental adaptation and evolution (Roger M. Keesing discusses contemporary materialist anthropologists, whom he terms cultural adaptationists, in Chapter 2 below). This definition of culture as a system of knowledge, because it is so basic to the content and scope of cognitive anthropology, will be the subject of thorough discussion in the introduction to Section I.

Cognitive anthropology did not, however, appear suddenly and fully developed on the anthropological scene in 1957, without connections with research in other areas of anthropology. Rather, this new approach was a development from research in two earlier anthropological research traditions. One of its sources was research in linguistic anthropology, particularly the line of research guided by the Sapir-Whorf hypothesis, which maintains that language and culture are wholly relative, and that culture is wholly shaped by language. Since relativity is still a major issue in cognitive anthropology, we will have occasion to consider it in some detail in Section II. But what is noteworthy at this point is the fact that it was from this linguistically oriented approach that the new field inherited the view that language and culture are intimately related and that the former provides a key to understanding the latter.

Although researchers in the new field resisted the idea that culture is determined exclusively by language, they nonetheless adopted the view that insights into the cultural orientation of society members could be achieved through analy-

sis of linguistic materials. Many studies in the middle 1950s and early 1960s, in order to discover how people in different societies conceptualize their natural environment, focused on words and their relations to categories of plants and animals. Because these studies dealt with folk botanical and zoological knowledge, the field at this earliest point in its development was dubbed *ethnoscience*. As researchers turned from studies of folk scientific knowledge to such other domains of cultural knowledge as law, medicine and religion, the field came to be called *ethnosemantics* or *ethnographic semantics*. These labels, although perhaps inelegant, aptly indicate the concern in this research with elucidating folk meaning. In addition to Goodenough, the anthropologists whose writings were most responsible for these early developments are Charles O. Frake (cf. 1962) and Harold C. Conklin (cf. 1962). The now classic articles of these writers, and several survey articles (Sturtevant 1964; Colby 1966; Kay 1970), are still the best sources of information on early ethnoscience and ethnosemantics.

The influence of linguistic anthropology and linguistics proper on the development of cognitive anthropology went beyond a basic orientation to the relationship between language and culture. More pragmatic borrowing also occurred. The methods and techniques developed by ethnoscientists and ethnosemanticists in their studies of cultural systems were modeled on procedures used in linguistics in analyzing the phonological (sound) and grammatical systems of languages. The language/culture relationship, which is still one of the most basic issues in cognitive anthropology, will be considered at length in the introduction to Section I, "Language, Culture, and Cognition," and the concepts and procedures used in the study of systems of cultural knowledge, as well as the question of their relativity, will be principal topics in Section II, "Folk Classification: Relativity and Universality."

Culture and Personality is the second anthropological subfield which contributed importantly to the formation of cognitive anthropology. In this subfield, which was an active area of research from the 1930s to the early 1960s, peaking in the middle 1950s, the primary research interest was individual psychology and its relation to culture (cf. Barnouw 1979; Bock 1980). Although this school of psychological anthropology was dominated by a concern with affective aspects of individual personalities, Freudian psychology being the main source of theoretical inspiration (see, for examples, such studies as Benedict 1934; DuBois 1944; Gladwin and Sarason 1953), cognitive aspects received some attention, particularly in late Culture and Personality writings, such as Anthony F. C. Wallace's book *Culture and Personality* (1961). As interest in the relation between culture and the emotional structures and processes of individual society members waned, interest in the relation between culture and the mental structures and processes of individuals increased, most importantly due to the influence of studies of cognition in cognitive psychology, the most important of which were Brunner et al., 1956 and Miller et al., 1960.

The individual human mind and the nature of the structures and processes located therein have been central topics of research in cognitive anthropology since its inception. Investigators have from the beginning been concerned with the issue of psychological validity—with whether or not the constructs and rules posited on the basis of analytical procedures (derived for the most part from linguistic models) are "psychologically real." Two studies of the American kinship terminology, published by Anthony F. C. Wallace and John Atkins (1960) and

A. Kimball Romney and Roy G. D'Andrade (1964), were particularly significant in the formulation of this issue. On the basis of independent analyses, different cognitive structures are proposed in these studies for the kin classification system. What was and continues to be at issue in considerations of these alternative solutions is the determination of which of them most closely represents the cognitive structures in the minds of individual society members. Decisions about the psychological reality of competing analyses can be made only through comparisons of the solutions with independent psychological and behavioral data and, as we shall see, there are a number of complex problems and issues involved in this research.

Cognitive anthropologists have also, in connection with their studies of psychological reality, confronted the problem of sharing and variability. This is a problem which was recognized in assessments of the treatment in most Culture and Personality research of the relation between the individual and his culture. A particularly incisive discussion appeared in Wallace's (1961) book, which still contains some of the best theoretical writing on the subject. The basic question stems from an apparent contradiction: culture is cognitive and therefore located in individual human minds, yet no two individual society members share exactly the same system of cultural knowledge. The issue then is, In what sense is culture a cognitive property of individuals, and in what sense is it a shared property of society members? Like the language/culture relationship, the culture/cognition relationship remains one of the central issues in cognitive anthropology and will be thoroughly considered in the introduction to Section I, "Language, Culture, and Cognition"; the problems of psychological reality and sharing and variability will be addressed specifically in Section IV, "Variability and Change."

Though the school of cultural anthropology whose origins in earlier linguistic and psychological approaches we have been considering is widely referred to as *ethnosemantics* and among some researchers as *ethnoscience,* there seems to be a growing consensus that the most suitable term for the field is the one used throughout the present discussion, *cognitive anthropology.* This label seems to capture the major issues and research interests that characterize the school as a whole, and it highlights a parallel between this anthropological subfield and subfields in other disciplines. There is a well-established subfield in psychology known as *cognitive psychology* (cf. Neisser 1976); there is a branch of sociology that is sometimes labeled *cognitive sociology,* although *ethnomethodology* is probably more widely used (cf. Cicourel 1972); and there is a camp of linguists who have recently adopted the label *cognitive linguistics* for their research program (cf. Lakoff and Johnson 1980). These names are not fortuitous, but reflect a convergence of interests and perhaps the beginnings of a truly multidisciplinary study of human cognition. The current level of development of this general enterprise, which is often referred to as *cognitive science,* has been summed up as follows:

> Overall, communication and cooperation between scientists interested in cultural systems of cognition, and those interested in artificial intelligence, formal and natural languages, the structure of the brain, and basic psychological models of cognition has already reached a promising level. All these areas of cognitive science are tightly interrelated: findings in each field impact directly on all the others. An organized cognitive science program promises a synthesis of wide applicability and major scientific relevance. (D'Andrade 1979:5)

SUBDISCIPLINES WITHIN CULTURAL ANTHROPOLOGY

Several other subdisciplines within cultural anthropology share with cognitive anthropology an interest in the workings of the human mind in social life. These approaches are similar to cognitive anthropology in that they adopt a mentalist perspective, but they differ in important ways that emphasize the distinctive orientation of cognitive anthropology. One of these other subfields is *structural anthropology,* dominated by the French anthropologist, Claude Lévi-Strauss, whose writings have had an enormous impact on anthropological thinking not only in Europe but also in the United States. As Keesing points out in his survey of theories of culture (Chapter 2), Lévi-Strauss's principal aim is to discover universal structures and processes of the human mind through an examination in a range of societies of the superficially distinct cultural organizations of particular domains—the most thoroughly studied of which have been kinship and myth. The mode of analysis used in inferring these shared abstract properties of mind are admittedly impressionistic and intuitive, but they have led to insightful statements about the nature of Culture (as opposed to individual cultures) that have aroused a great deal of interest (and controversy) and stimulated a vast amount of anthropological research.

Cognitive anthropologists are also concerned with cognitive universals, but they are interested not so much in the nature of Culture as in universals in cultures. The approach of cognitive anthropologists is tightly tied to empirical reality. Data from particular societies are viewed in cross-cultural perspective, universal structures and processes are isolated, and inferences are drawn about their historical and evolutionary development. To return to our earlier example, Berlin's discussion of ethnobiological ranks is based squarely on the empirical analysis of plant classification systems, not only the Aguaruna system but a large number of others as well. His conclusion that ethnobiological classification systems are universally organized in terms of the same hierarchical ranks is a conclusion about the cultural organization of these systems everywhere, not just in one society and not just at an abstract level. Berlin and others, as we will see in Section II, particularly in the chapter by J. W. D. Dougherty (Chapter 8), further argue that ethnobiological classification systems develop in fixed sequences—that generic rank categories are the first ones encoded, followed by life form and/or specific rank categories, and so on. For the present, however, the important point is that structural anthropology and cognitive anthropology share an interest in conceptual universals but adopt distinct orientations to them and different strategies in studying them.

Another subfield with which cognitive anthropology shares certain interests is *symbolic anthropology.* As Keesing states in his theoretical overview (Chapter 2), this school of cultural anthropology, like cognitive anthropology, takes culture to be a shared system of symbols and meanings. Of the major writers in symbolic anthropology, two need to be considered here. One is Clifford Geertz, who maintains that symbolic meaning exists in social interactions, not in individual minds, and that the business of the anthropologist is to interpet this meaning, which is communicated between real society members in real social situations, in "thick descriptions"—ethnographic accounts which describe cultures as they are situated in the actuality of ongoing social life. The other is David M. Schneider, whose position—contrary to Geertz's—is that symbolic meaning exists at a sepa-

rate, abstract level independent of the level of situated social interaction, and that cultural symbols should be analyzed apart from and without reference to the actuality of social life.

In cognitive anthropology, culture is viewed not only as a public symbol system used to convey meanings between minds, but also, as we have seen, as a private cognitive system whose ultimate locus is individual minds. It is also a general proposition in cognitive anthropology that culture should be understood in its intimate interconnectedness with social behavior, not in isolation from it. These contrasting positions distinguish cognitive anthropology from symbolic anthropology and can be illustrated by returning to another of our earlier examples. In providing an account of Western Apache "wise words," such as "Butterflies are girls," Basso is interested not only in examining "wise words" as cultural symbols, but also in the cognitive processes of implication and inference that individuals employ in creating and interpreting the metaphorical meanings communicated by "wise words." And he does not limit his study to an analysis of the cultural meanings of "wise words," but shows in very fine detail how they are actually used in social situations to accomplish acts of mild criticism indirectly. The interrelations between public symbols and private cognitions in culture and the interconnectedness of culture and social behavior are basic issues in cognitive anthropology to which thorough consideration will be given in the introduction to Section I.

Perhaps the subfield which is most similar to cognitive anthropology in its range of interests is the *Ethnography of Speaking,* which in addition to being a subfield of cultural anthropology is also a part of the wider interdisciplinary field of sociolinguistics. The anthropologist who originated this school of research and who is still its most influential member is Dell Hymes (cf. 1962; 1972). Like the field of sociolinguistics as a whole, the ethnography of speaking is concerned with the relation between language and social context. Much ethnography of speaking research is aimed at discovering the social meaning conveyed in the contextually situated use of language. The basic notion is that the social meaning communicated in language usage is determined as much by the social context of speaking as by the semantic content of the words and sentences that are spoken. The approach adopted by most researchers in this subfield is to analyze social meaning in terms of the social factors (for example, sender, receiver, audience, setting, topic, etc.) that characterize situations of speaking. This approach is more fully discussed by Brian Stross in Chapter 1.

Context is also a topic of major interest in cognitive anthropology, but here it is more broadly conceived to include nonverbal behaviors (e.g., gestures, postures, movements, and facial expressions) as well as the verbal behavior of speaking. The emphasis in cognitive anthropology is on the reciprocal influences of context and behavior—on how contexts constrain the appropriateness of behaviors and on how behaviors guide the interpretation of contexts. The approach is a dynamic one in which the focus is on the accomplishment by participants of ongoing activities—on how participants (and the anthropologist who has learned their culture) are able to produce and interpret the significance of events in their society. This is illustrated by the example from McDermott et al. of the teacher and six schoolchildren engaged in a reading lesson. At one point in the lesson, the teacher and children move into position to begin reading, but are distracted and briefly move out of this postural configuration. These

movements are followed by a dance—a brief series of subtle, coordinated movements—that re-establishes the reading context by drawing the participants back into the postural configuration for reading. When this configuration is re-established, reading resumes. As this example shows, the dynamic coordination of context and behavior in organizing and interpreting social meaning can occur without the utterance of a single word.

HOW THIS BOOK IS ORGANIZED

Coverage of the topics and issues described here as distinctive to cognitive anthropology will be organized in the five sections of this anthology in the following way.

Brief but comprehensive introductions to language, culture, and cognition will be presented in Section I. The three chapters in this section will provide overviews of the nature of language, theories of culture, and research on cognition, and the introduction will discuss the relationship between language and culture and describe language and culture as cognitive systems. In the introduction to this section, as in the introductions to the other sections of the book, basic concepts and problems will be described, and the chapters comprising the section will be introduced and placed in relation to each other and to the major theoretical and research concerns of cognitive anthropology.

Folk classification systems, the type of cognitive system which has so far been most thoroughly studied, will be the focus of attention in Section II. The methods and procedures developed in ethnosemantic research to discover the internal conceptual organization of classification systems will be surveyed, as will recent research dealing with questions of relativity and universality in these systems. In Section III, two types of meaning in classification systems will be considered: the extension of meanings within categories and the transfer of meanings across categories. And the concern in Section IV will be with the problems of synchronic variability and diachronic change in classification systems—with how the cognitive systems of individuals vary in societies and how this diversity serves as the basis of change.

The last section of this anthology, Section V, will focus on cognition in relation to context and behavior. Cognitive systems other than classification systems, some of which are not directly encoded in linguistic structures or can be verbalized only with difficulty, will be emphasized, e.g., cognitive structures and processes in decision-making and problem-solving. Other major issues which will be dealt with are the interconnectedness between cultural systems of knowledge and actual behavior and observable events in the physical world, and the role of context in the production and interpretation of behavior and events.

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I

Language, Culture, and Cognition

In this section, we will be concerned with general perspectives on language, culture, and cognition. The three chapters that comprise the section provide overviews of the major concepts involved in the study of each of these topics. In Chapter 1, Brian Stross discusses the nature of language. He focuses both on how it is organized as an abstract system, or code, for communicating meanings in sound and on how it interrelates with social factors in its actual use by communities of speakers. In Chapter 2, Roger M. Keesing surveys the theories of culture that are currently important in cultural anthropology. There is a good deal of disagreement among anthropologists about the nature of culture; Keesing provides an overview of these discrepant theories and offers some suggestions for a "conceptual untangling." In Chapter 3, Anthony F. C. Wallace discusses cognition from the perspective of modern psychology and, in the process, indicates the relevance of this research to anthropological studies of culture. These overviews supply background information about language, culture, and cognition that is prerequisite to considering the detailed studies in subsequent sections of this book. In this introductory discussion, an attempt will be made to highlight background information and to interrelate the separate considerations of language, culture, and cognition in the three overviews.

DEFINING LANGUAGE

We can begin our consideration of language where Stross begins his—with his nine-year-old nephew's definition of language as "words and the way we string them together and pronounce them to communicate ideas." This is right on the money, regardless of the age of the linguist. Although this definition needs elaboration and refinement, as Stross points out, the crucial elements and their interrelations are all indicated. Words are the central elements in language, and they are pronounced in certain ways and combined in certain ways to communicate ideas. In expanding on his nephew's definition, Stross provides a concise but very thorough characterization of language. The principal contribution of these introductory remarks will be to supply additional background information that will be needed in relating language to culture and cognition.

That language is a "symbolic meaning system" is a notion that requires some

particular background information. The significance of this notion will be described subsequently in the discussion of culture, which is also often characterized as a symbolic meaning system. What is meant by these characterizations of language and culture is that both are systems in which symbols are the basic means by which one individual communicates an idea to another. The nature of symbols is the specific concern of researchers in the field of *semiotics,* the study of signs. A sign has two aspects, a form that signifies and an idea that is signified. Ferdinand de Saussure, the father of both structural linguistics and semiotics, referred to the former as the *significant,* or signifier, and the latter as the *signifié,* or signified (cf. Culler 1976, pp. 9–11). Here we will follow current usage and refer to the formal aspect of the sign as the sign vehicle and the conceptual aspect of the sign as the signaled entity (cf. Silverstein 1976). Sign vehicles are perceivable objects, actions, or events that stand for or call attention to signaled entities, which may range from physical objects (ball, flower) and perceptual qualities (red, round) to nonexistent entities (ghost, Superman) and abstract ideas (relativity, fair play).

Three types of signs are generally distinguished: icons, indexes, and symbols. An *icon* is a sign whose sign vehicle and signaled entity are connected by virtue of a relationship of similarity. The sign vehicle resembles its signaled entity; it looks or sounds like what it represents. The prototypic example of an icon is the statue in the park that represents an important historical personage. In American Sign Language (ASL), the language of the deaf in the United States, the sign meaning *milk* is made with one hand held in front of the body in roughly the shape of a fist, with the fingers and thumb opening and closing several times; it is an iconic sign: it looks like a hand milking a cow (Markowicz 1977, p. 12). Spoken languages also employ iconic signs. Onomatopoeic words (words that imitate the characteristic sounds made by the entities they represent) are examples: the barking of dogs is onomatopoeically represented as *bow wow* in English, *ouâ ouâ* in French, *wang wang* in Chinese, and *hav hav* in Turkish; doorbells ring *ding dong* in English, *bim bam* in German, and *çın çın* in Turkish.

An *index* is a sign whose sign vehicle indicates its signaled entity through an existential relationship. The sign vehicle stands in a relationship of spatiotemporal contiguity with the signaled entity. It is tied to the situation of usage in which the signaled entity is present, and the signaled entity can only be indexed in the situation of usage. To cite the classic examples of indexical signs, clouds are an index of rain and smoke is an index of fire. Indexical signs in languages include expletives and personal pronouns. For example, a carpenter who, upon hammering his thumb rather than the intended nail, utters an expletive such as "Ouch!" or "Gosh!" or "Oh, darn!" (there are, of course, other possibilities) is indexing a particular inner state. The personal pronouns *I* and *you* are indexes that point to the individuals who occupy the roles of speaker and hearer in the situation of usage. In the sentence, "I gave you the bribe," the pronoun *I* indexes the speaker as the doer of the action; *you* marks the hearer as the receiver of the action. Signers of ASL literally point at interlocutors (with an index finger) in indexing the occupants of the signer and watcher roles. A signer indicates *I* or *me* by pointing at his or her own chest and indicates *you* by pointing at the watcher's chest.

A *symbol* is a sign whose sign vehicle bears an arbitrary relationship to its signaled entity. The relationship does not depend either on a formal similarity

between sign vehicle and signaled entity or on their contiguous occurrence in the situation of usage. There is no inherent tie between sign vehicle and signaled entity; they are related arbitrarily solely as a matter of convention. There is no reason why the beginning and end of college class sessions are signaled by the ringing of a bell; they could just as well be (and in some institutions perhaps are) signaled by the clanging of a chime, the honking of a horn, or the banging of a drum. The same is true of traffic signals: the red light symbolizing "stop," the yellow light symbolizing "caution," and the green light symbolizing "go" are simply arbitrary conventions. Red could just as well signal "go," and other colors, such as purple and orange, could serve as sign vehicles just as well.

Words as Symbols

It is a general principle of linguistics that words in human languages are symbols whose connection with their referent entities is purely arbitrary. There is no inherent reason why certain small herbaceous plants are referred to in English by the collection of vocal noises that comprise the word *flower* or why certain spherical rubber objects used in games are referred to by the set of sounds that comprise the word *ball.* Totally different vocal symbols are used in referring to these objects in other languages; in Turkish, for example, they are referred to as *çiçek* and *top.* This is not to say that iconic and indexical signs do not occur or are unimportant in human languages. The position of most linguists is that, while iconic and indexical signs do occur and are of some importance, they are much less important than symbols, which are central in language. It is for this reason, then, that language is described as a symbolic meaning system—it is a sign system composed mainly of symbols that are used to communicate meaning from one mind to another.

We will take up the topic of the symbolic nature of words in more detail shortly, but at this point it is important to note that both iconic and indexical signs are often conventional to a significant extent. Onomatopoeic words, as we have seen, are imitations or representations of sounds that occur in nature. But there is a highly significant arbitrary component in this iconic relation between sign vehicle and signaled entity. Note that, although the characteristic sounds of the various animals are the same around the world, the onomatopoeic words that imitate the sounds differ from language to language: *bow wow* versus *ouâ ouâ* versus *wang wang* versus *hav hav.* So while the relationship between onomatopoeic words and what they represent is based on a general similarity, the relationship is also conventional to the extent that the imitations (the sounds) are particular to each language. The ASL sign for *milk,* although generally iconic in that it looks like what it represents, also has an important conventional aspect. The resemblance of the milk sign to the act of milking a cow is irrelevant to a child who knows milk only as a drink from a carton. To such a child, the relationship between the sign vehicle for milk and the signaled entity *milk* is wholly arbitrary (cf. Markowicz 1977, p. 12).

Indexical signs, which indicate their entities through existential relationships in situations of usage, frequently take on conventional associations such that they are used when the signaled entity does *not* occur contiguously in the utterance situation. A little boy who pretends to hit his finger while pounding with a toy hammer and screams "Ouch!" is not indexing a real pain, but rather symbolizing a nonexistent pain in a conventional manner. The schoolteacher

who shouts "Fire!" during a fire drill is not drawing attention to a fire in the situation of his or her utterance but is using a conventional index in simulating what would happen in the event of a real fire. And the boy who cries "Wolf!" uses a cultural convention in conveying a danger that does not actually exist in the context of his shouting.

Not only are there indexes that function as symbols, but there are others that function both as indexes and symbols at the same time. These are known as *referential indexes* (cf. Silverstein 1976). To return to our example, "I gave you the bribe," it may now be noted that the personal pronouns *I* and *you* operate simultaneously as referential symbols and situated indexes. They signal referential values in the proposition coded in the sentence (first-person referent "gives to" second-person referent) at the same time that they indicate that the speaker of the situated utterance was the giver and the hearer was the receiver. The schoolteacher who shouts "Fire!" and the boy who cries "Wolf!" are also using referential indexes. In both examples, the speaker indicates the presence of danger in the situation of utterance (even when the danger is fictitious), and refers to a specific dangerous entity.

Words, more technically termed *lexemes* or *lexical items,* are the symbols in human languages that encode arbitrary but constant relationships between sign vehicles and signaled entities. They are, as Stross says, the lifeblood of language. They are the means by which meaning and form are associated. We will take the meaning side of this association first. The meaning of a word depends on properties, or features, of the entities to which it refers. Properties of entities in the nonlinguistic world of experience or imagination are utilized in forming concepts and, as a result, the meaning associated with a word is a conceptual abstraction. Properties of referent entities are encoded in language as abstract conceptual distinctions, generally termed *semantic features* or *components.* To take a specific example, the meaning of the word *man* in English depends on properties of the entity referred to—a creature or being who is adult, male, and human. These properties of the referent entity are the abstract semantic features that define the meaning signaled by the English word *man.* Not all the concepts associated with words are abstracted from referents that actually exist in the physical world of experience. The word *Superman,* which is currently a very meaningful word in English, refers to a kind of being (and is even the name of a particular being) who is adult, male, and superhuman. These properties are the features that define the meaning of *Superman,* even though its referent entity does not exist in the world of experience, but only in an imagined world depicted in movies and cartoons. Semantic features will be a major topic in the introduction to Section II.

In addition to a meaning, each word symbol also has an associated form. Word forms are sequences of sound *segments* or *phonemes,* which are conceptual units in the phonological or pronunciation system of the language. These abstract mental constructs are manifested or realized in actual spoken utterances as vocal sounds. Because a thorough discussion of the major phonological concepts is presented in Stross's chapter, they will be only briefly illustrated here by describing a single sound segment, the /*k*/ phoneme, in the phonological system of English. This conceptual category is defined by the conjunction of three distinctive features: by the momentary stoppage of the air stream that flows from the lungs through the mouth and nose in producing all sound, by the location of this stoppage

in the back of the mouth (brought about by contact between the back of the tongue and the soft palate, or velum), and by the absence of voicing, or vibration, of the vocal cords as the stoppage is released. The English /*k*/ phoneme is therefore distinguished as voiceless, velar, and stopped. However, the actual pronunciation of the /*k*/ in *kin,* to choose a word that is a favorite of cognitive anthropologists, includes additional nonphonemic (nondistinctive) features of sound. The actual phonetic manifestation will include idiosyncratic features that result from the individual speaker's voice qualities and emotional state and, more importantly, regular features that are produced on the basis of general rules for English pronunciation. The /*k*/ in *kin,* for instance, is realized not only as voiceless, velar, and stopped, but also as aspirated—that is, as being pronounced with a puff of air accompanying the stop release. This occurs not as a distinctive property of the /*k*/ phoneme, but rather as a nondistinctive aspect of English pronunciations: it is a general rule of English phonology that all voiceless stops occurring at the beginning of a word are aspirated.

The important point here is that the relationship between the sequence of phonemes associated with words and their phonetic manifestation in the physical world parallels the relationship between the meanings associated with words and their referent entities in the world of experience and imagination. Words are symbols that encode an arbitrary relationship between meaning and form, both of which are conceptual abstractions within language defined with respect to properties of the nonlinguistic world. This distinction between aspects of meaning and form that are part of the internal structure of language (and thus "in the head") and those that are external to language (and thus "in the world") is central in cognitive anthropology. Borrowing from the linguistic terminology *phonemic* and *phonetic* for conceptual versus physical properties of sound, cognitive anthropologists have coined the terms *emic* and *etic* in studies of culture and its relationship to social behavior. Culture, like language, is viewed as being an emic, or internal, conceptual system; social behavior is an etic, or external, manifestation. This cognitive view of culture, which was considered briefly in the General Introduction (see p. 4), is a topic to which we will return very shortly.

Words as Syntactic Units

As Stross describes in detail in his article, in addition to symbolizing meaning-form correspondences, words also function as units in the syntactic or grammatical structure of language. Words are classed as members of grammatical categories (noun, verb, adjective, and so on) and, as such, are subject to the syntactic rules that combine them in sentences. Syntactic rules account for the fact that not every combination of words constitutes a grammatical sentence. "John lived in a big house" is a grammatical sentence in English, but "John lived in big a house" is not because it violates the English syntactic rule that articles come before adjectives in noun phrases. A Turkish sentence ordered in this way (*Ahmet büyük bir evde oturdu,* which is literally, "Ahmet big a house-in lived") is grammatical because Turkish syntactic rules require that indefinite articles immediately precede the nouns they modify.

Syntactic rules are mental constructs that speakers use to distinguish and produce combinations of words that are grammatical sentences. A sentence is grammatical if it is intuitively regarded as well formed by a native speaker of

the language. A linguist's grammar of the language attempts to account formally for these intuitive judgments. It is an explicit statement of the syntactic rules that specify which combinations of words are acceptable sentences in the language and which combinations are not; it is a description of the intrinsic structure of grammatical sentences in the language. The rules are a finite set (that is, they could all be specified in an exhaustive list) and are productive or creative in the sense that they account for an infinite number of sentences. A grammar of a language is a finite set of rules, the most concise and explicit that can be formulated, that has the ability to generate the infinite set of sentences that speakers of the language regard as grammatical and none that they regard as ungrammatical.

Linguistic Competence Versus Linguistic Performance

Another basic concept in linguistics is the distinction between competence and performance. Linguistic competence is the knowledge that a native speaker has of his or her language—that is, knowledge of syntactic, semantic, and phonological units and the rules for their organization in sentences. *Competence* is the ability or capacity of the speaker to produce and understand sentences that are syntactically, semantically, and phonologically acceptable; it is distinct from *performance,* which is the speaker's actual use of this knowledge in the production and interpretation of sentences. Competence is an underlying cognitive ability, whereas performance is the overt manifestation of this ability in actual behavior. Performance is the use to which competence is put in speaking. As a result of such factors as mistakes, hesitations, false starts, self-corrections, and general psychological processing limitations, however, actual speech is only an imperfect reflection of the speaker's underlying linguistic competence (Chomsky 1965, pp. 3–4). Linguists are concerned almost exclusively with linguistic competence and very little with the encoding and decoding of language in performance. Linguists argue that their research priorities are ordered in this way because competence logically precedes performance; what a language is must be known before how it is used can be understood.

Semantic Analysis

In linguistics, the meanings of words and sentences have generally been analyzed apart from the contexts in which they are used. The focus of semantic analysis in linguistics has been on referential meaning—the meaning encoded in word symbols by virtue of their arbitrary association with referent entities. This is also the variety of meaning that has generally been the focus of analysis in studies of folk classification systems in cognitive anthropology, as will be seen in detail in Sections II, III, and IV. However, as Stross points out in his article, more than referential meaning is conveyed in the actual use of language in social contexts. Utterances that have exactly the same referential meaning can be used in context to convey very different social meanings. For example, when spoken by a movie viewer to his companion, the sentence, "I can't see the screen," is a report of certain difficulties he is experiencing in watching the movie; but when it is said by the same viewer to the person seated in front of him, it is a request of this person to take off his hat, move to another seat, sit lower in his seat, or something of the sort.

The reverse relation between language and context also occurs quite com-

monly: utterances that have different referential meanings can be used to convey the same social meaning. A passenger in an automobile, for instance, can request the driver to stop, by uttering any of the following sentences: "Stop the car!" , "Can you stop the car?", "There's a red light at the corner," or "I'm getting out right now." Reports of states of affairs and requests for changes in states of affairs are types of speech acts, acts that are accomplished through the use of language (cf. Searle 1969). What is done (as opposed to what is simply said) in uttering a word or sentence is understood by participants on the basis of social factors in contexts of usage. Stross describes seven of the most essential of these factors in his chapter: sender, receiver, message form, message channel, topic code, and setting (cf. Hymes 1972). As described in the General Introduction, the relation between language and social context is a central topic of study in sociolinguistics, most especially in the subfield of sociolinguistics known as the *ethnography of speaking.* It is also an important concern in recent research in cognitive anthropology. In Section III, we will examine research concerned with speech acts and social contexts (Chapters 11 and 12), and in Section V we will look at research dealing with the relationship of context to other (nonlinguistic) behaviors (Chapters 17, 18, and 19).

DEFINING CULTURE

There is much less agreement in anthropology about the nature of culture than there is in linguistics about the nature of language. In Chapter 2, Roger M. Keesing provides a lucid survey of the most influential theories of culture in contemporary anthropology; he then constructs a theory of his own to rectify the inadequacies and contradictions he finds in those theories. Keesing bases his theory on the assumption that language and culture are parallel ideational codes underlying the realm of observable events. By tracing the details of this parallel, he is able to resolve some of the important disagreements among contemporary theorists and, at the same time, to formulate a revised theory of culture that builds on the insights of those authors.

Culture, it is argued by many anthropologists, most notably Clifford Geertz, is a symbolic meaning system. Like language, it is a semiotic system in which symbols function to communicate meaning from one mind to another. Cultural symbols, like linguistic symbols, encode a connection between a signifying form and a signaled meaning. Symbols are public. They are perceivable objects, actions, or events in the world that signal meanings between the minds of individual members of society. Geertz argues that cultures therefore do not exist *in* the minds of individuals but rather *between* them.

Other anthropologists, including most prominently Ward H. Goodenough, maintain that culture is an idealized cognitive system—a system of knowledge, beliefs, and values—that exists in the minds of individual members of society. Culture, according to this view, exists in the "ideational order of reality." It is the mental equipment that society members use in orienting, transacting, discussing, defining, categorizing, and interpreting actual social behavior in their society. It is the means by which society members generate appropriate social behavior and interpret appropriately the behavior of others. Social behavior occurs in the actual physical world, in the "phenomenal order of reality." It is the things people actually say and do.

These theoretical perspectives differ most significantly with respect to where they locate culture. For Geertz, culture is a system of shared symbols that exists outside the individual. Although each individual society member has his or her own perception of it, culture, as such, is independent of individual human minds. Culture is an autonomous system of symbols that the individual is free to deal with as he or she sees fit—to use as a guide, to manipulate to advantage, to attempt to change. Because culture is viewed as having an independent existence apart from individual human minds, the anthropologist working from this perspective need not be concerned with whether society members have the same, similar, or radically different perceptions of the society's culture.

For Goodenough, on the other hand, culture exists in the minds of individual human beings. Individuals, however, do not share exactly the same model of their culture, just as they do not share exactly the same model of their language (that is, individuals differ in dialect and idiolect). Because of differences in biological heritage, unique personal histories, and the different roles individuals occupy in society, individual society members have varying cognitive models of society's culture. This being the case, the anthropologist's description of the society's culture is an idealized composite of the cognitive models of individual society members.

Cultural Competence Versus Cultural Performance

As Keesing cogently argues, the contradictions between these two theories—one holding that culture is an autonomous symbolic meaning system and the other holding that it is an idealized cognitive system—can be resolved and a more adequate theory of culture can be formulated if the distinction between competence and performance employed in the study of languages is introduced into the study of cultures as well. In the course of growing up, each individual constructs a model or theory of the society's culture. This is cultural competence, one's knowledge of the culture. Because it is a personal culture model, it is only partially shared by other members of the society. The individual's model of the culture does not comprise his or her entire cognitive world; it is only a portion of it. In addition to cultural competence, the cognitive world of the individual includes idiosyncratic knowledge, beliefs, and values that are the result of his or her unique life experiences. The individual constructs a culture model on the basis of experience in the society; as a consequence, he or she may regard this internal construction as having an external counterpart. So, although the individual may perceive the culture as being an autonomous system of symbols that he or she can use, manipulate, or change, it is ultimately a mental construct located only in individual minds.

The anthropologist's description of a culture is an idealized composite abstracted from the diverse models of individuals. In this regard, culture theory closely parallels linguistic theory, which is "concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows his language perfectly. . . ." (Chomsky 1965, p. 3). Language and culture are located in individual human minds, but the descriptions of linguists and anthropologists are of the perfect cognitive models of ideal individuals in completely homogeneous communities. This assumption of homogeneity is basic to most of the studies in this volume, but in Section IV the focus will be on linguistic

and cultural heterogeneity when we examine recent research on variability and change.

Cultural competence is put to use in generating and interpreting what Keesing calls "sociocultural performance." This is the overt manifestation in the physical world of underlying cultural competence. It is real people engaging in real behavior—transactions, events, and exchanges. An aggregate of people who recurrently interact on the basis of cultural structures and processes do so in a patterned fashion. These social patterns, which Keesing terms sociocultural systems, are the realization of cognitive systems in particular environments and, as such, are affected by processes of adaptation and change. To cite concrete examples, this distinction is between cultural constructs and principles specifying where to reside, when to plant wheat, whom to marry, and how to put a curse on an enemy, and patterns of actual residences, fields of wheat, marriages, and curses.

Cultural competence is conceptually distinct from, but closely interconnected with, sociocultural performance. Although linguists, as we have seen, have for the most part studied only linguistic competence and have devoted little attention to performance, Keesing argues that anthropologists should not adopt a parallel strategy. Cultural competence and sociocultural performance are intricately interconnected, and they must be examined together in their mutual interconnectedness if a full understanding of either is to be attained. The interconnectedness of cultural constructs and social behavior is given considerable attention in Section V.

To this point we have been discussing language and culture as parallel cognitive systems. Cultural competence is viewed as logically parallel to linguistic competence in Keesing's theory of culture. But the relationship between language and culture, between the cognitive system used in generating and interpreting speech behavior and the cognitive system used in generating and interpreting social behavior, is more than a parallel or an analogy; it is a system-subsystem relationship. Language and culture are not independent or mutually exclusive cognitive systems composed of analogous structures and processes. Rather, culture is a wider system that completely includes language as a subsystem. Linguistic competence is one variety of cultural competence, and speech behavior is one variety of social behavior. The relation of language to culture is thus a relation of part to whole.

DEFINING COGNITION

Cultural competence, including linguistic competence, does not comprise the entirety of the individual's cognitive world. As we have seen, each individual's overall cognitive system includes, in addition, many purely idiosyncratic and private cognitive structures and processes that constitute basic elements of his or her unique personality. In his review of research on cognition in Chapter 3, Wallace describes "mediating schemata," conceptual abstractions stored in the brain that mediate between stimuli received by the sense organs and behavioral responses. Schemata (the plural of *schema*) are abstractions that serve as a basis for organizing information. They have both structural and processual aspects: they are fixed sequences of interrelated elements that provide frameworks

for, e.g., the recognition and processing of information in perception and categorization, the organization of information storage in memory, and the reorganization of information and the filling of gaps in information in recall (Rice 1980, p. 153; also Tyler 1978, p. 109). And schemata differ in complexity, ranging from simple stimulus-response sequences to complex hierarchic organizations of simpler schemata (a detailed discussion of different types of schemata can be found in Tyler 1978, pp. 109–117).

LINGUISTIC AND CULTURAL SCHEMATA

Schemata also differ in their distribution in populations: some are universal, some are idiosyncratic, and some are cultural. Universal schemata are shared by all humans; they include structures and processes in perception, memory, and language that are uniform in the species as a result of innate faculties of the mind and/or inherent divisions in the natural world (we shall take up the question of innateness in the acquisition of language and culture in a moment; the question of cognitive universals will be considered in detail in Section II). Idiosyncratic schemata are unique to particular individuals; they include mnemonic devices invented by individuals to remember names, dates, and telephone numbers; habit structures developed by individuals for organizing recurrent or routine personal activities; ideas or beliefs held by individuals as the result of unique or unusual experiences; and wholly original and creative concepts formulated by individuals to solve problems, meet needs, beat the system, or improve social conditions.

Cultural schemata are neither unique to particular individuals nor shared by all humans—they are shared by the members of particular societies. Examples of cultural schemata include classification systems of plants, animals, material objects, and kinsmen; rules for assigning residence location and descent group membership; procedures for diagnosing illness and disease; constructs used in mathematical computation and deductive reasoning; strategies for making legal decisions and solving technical problems; and rules for behaving appropriately in specific cultural events.

Linguistic schemata, which, as we have seen, are a subset of cultural schemata, include the abstract categories of meaning associated with words and the abstract categories of sound (phonemes) that in sequences constitute the formal representations of words, as well as the phonological rules for combining phonemes in acceptable sequences and the syntactic rules for combining words in grammatical sentences. It is with cultural schemata that cognitive anthropologists are primarily concerned. Examples of cultural schemata are described throughout this volume—classification systems, which have received the greatest amount of attention to date are the principal type considered in Sections II, III, and IV, and other types appear in Section V.

Many cultural schemata are or can be represented by linguistic schemata. Constructs for identifying and classifying plants, kinsmen, diseases, and events, for example, are generally encoded in words, and principles for diagnosing illnesses, for postmarital residence, for descent group membership, and for arithmetical calculation and logical reasoning are often overtly verbalizable. But this is not true for all cultural schemata. Cultural constructs are frequently covert, and cultural principles are often difficult or impossible to express in language.

In fact, many constructs and principles are not only covert and unverbalizable, but are unconscious in the sense that they are out of the individual's awareness, even though they guide his or her behavior. This is, of course, true of linguistic schemata as well. Although the phonemes of a person's language and the syntactic rules for forming certain types of sentences (e.g., passives) are for the most part out of the individual's conscious awareness, the individual nonetheless has knowledge of these categories and principles and utilizes them in speaking. Parallel examples of unconscious cultural schemata are the covert (unlabeled) categories that occur in classification systems, the tacit procedures and strategies that are employed in certain kinds of decision-making and problem-solving, and the implicit distinctions that are utilized in recognizing or negotiating certain events and behaving appropriately in them.

Wallace discusses another issue of some importance in his chapter. This is the question of how cognitive schemata are formed and maintained. Linguistic and cultural schemata are for the most part not taught. Adults do not normally explain the operation of syntactic rules or the organization of classification systems to children. The specific role played by learning through experience with the external world in the acquisition of linguistic and cultural competence is not yet known; all that seems to be required is exposure to the actual speech and social behavior of adults. Children raised in isolation with little or not exposure to adult speech and behavior do not acquire linguistic or cultural competence.

But learning through experience is only one process contributing to the formation and maintenance of linguistic and cultural schemata. Wallace describes two additional processes. One is acquisition through innate faculties of the human mind. These are capacities for constructing cognitive schemata that are activated by experience in the external world. They are species-specific products of millions of years of evolution that are wired into the human brain and are genetically programmed to operate in connection with experience to develop abstract cognitive structures and processes (see Chomsky 1975, Chapter 1, for a discussion of innateness in the acquisition of cognitive schemata). The second additional process Wallace discusses is what he calls "autistic thought." This is the capacity to reorganize cognitive structures and processes developed through learning and innate faculties—to create new schemata through recombining elements in already acquired schemata. As Wallace points out, this is the cognitive mechanism by which alternate schemata are invented by individuals. How cultural schemata are formed and maintained in social groups is an important topic to which we will return when we take up the issues of variability and change in Section IV.

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1

The Nature of Language

BRIAN STROSS*†

LANGUAGE AS CODE

We all have some notion of what language is. My nine-year-old nephew defines language as "words and the ways that we string them together and pronounce them to communicate ideas." Not bad, for this embodies in simplified terms the technical categories "lexicon," "syntax," "phonology," and "semantics." But then, like so many definitions, it really doesn't tell us much by itself. Let's begin instead with a slightly more precise characterization of language—one that can be dissected a bit more easily:

> Fewer than one hundred sounds which are individually meaningless are compounded, not in all possible ways, to produce some hundreds of thousands of meaningful morphemes which have meanings that are arbitrarily assigned, and these morphemes are combined by rule to yield an infinite set of sentences, having meanings that can be derived.[1]

With this kind of communication system we are able to transmit emotionally neutral messages that have never been sent before; messages that can be correctly understood by anyone using the same code; messages referring to things remote in space and time; messages that can be quite complicated, yet readily understood even in the absence of face-to-face interaction. A communication system with these capabilities sets us apart from all other animals. It enables us to develop writing, mathematics, telephones, airplanes, radios, and atomic bombs, to name but a few cultural artifacts that would never exist but for language.

We bring many cognitive skills to the task of learning a language, as well as the motivation to learn it. These skills are inborn in the sense that they unfold in a genetically controlled sequence. Only extreme physical handicaps can prevent a human child from learning to speak; yet no other animal has ever mastered a human language, although recent experiments with chimpanzees demonstrate some remarkably language-like capabilities. . . . What we don't bring to the task of language acquisition is an innate knowledge of sound discriminations that must be made, of permissible sound sequences, of word forms and the meanings associated with them, and of the ways in which words must be combined to convey intended messages. These things are specific to the language being acquired and must be learned. In learning a language, the child uses his wired-in but constantly developing cognitive skills to perceive and extract regularities from the welter of linguistic input to which he is

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† Source: *The Origin and Evolution of Language* (Dubuque, IA: William C. Brown Company, Publishers, 1976), pp. 2–19.

[1] Roger Brown, *Social Psychology* (New York: Free Press, 1965), p. 248.

exposed. Eventually he will have internalized a linguistic system quite like that of his parents.

An individual's internalized linguistic system, the knowledge enabling him to produce and understand an infinite number of sentences, is his linguistic *competence.* This linguistic competence is sometimes called a competence grammar. The sentences that he actually produces, whether grammatical or not, constitute his linguistic *performance.*[2] Competence is not available for direct observation. It must be inferred from performance.

The distinction between competence and performance is similar to that which separates language from speech. *Language* is the system itself, a system of inferred rules upon which behavior is based. *Speech* is the vocal manifestation or application of this linguistic system. Since it is difficult to imagine speech apart from social contexts in which it occurs, we will ultimately have to deal directly with the interaction situation, with speech in its natural habitat. But let's not open the door to that just yet.

To simplify exposition, we shall continue the convenient fiction that language is a referential system only: that it provides the linkage between the sounds that we hear and the objects, events, qualities, and ideas to which the sound sequences refer. It would be oversimplification, however, to suggest that the linkage between sound and referent is direct or uncomplicated. To the contrary, the relationship is indirect and complex, going from a very concrete level of organization—sound manifestations—to a highly abstract level—concepts. The abstract conceptual level is linked in turn to the concrete world of objects and events by a complex process known as perception. Think of the words "justice," "yesterday," "unicorn," "Horatio," "ache," or "Jupiter," and it becomes apparent that referential meaning is really represented as concepts, configurations, or entities in the mind, rather than out there in nature.

PHONOLOGY

Speech originates in the brain. A complex network of nerves leads from an area in the dominant hemisphere to control operations of the tongue and other articulators, the size and shape of the vocal tract, and the vocal cords. When you breathe or talk, air passes through the glottal opening between vocal cords as well as through your throat *(pharyngeal cavity)* and mouth *(oral cavity)* or nose *(nasal cavity).* Speech sounds come about through modifications of the breath stream on its journey through these areas.

When the vocal cords close your glottis, creating vibrations, the vowels and consonants of speech are said to be *voiced.* You can feel this by touching your Adam's apple. An open glottis, on the other hand, produces *voiceless* sounds (e.g., *p, t, k, s, f*). The vocal cords, varying in length

[2] For a more detailed and broadened discussion of the competence-performance distinction see Dell Hymes, "Competence and Performance in Linguistic Theory," in *Language Acquisition: Models and Methods,* eds. R. Huxley and E. Ingram (New York: Academic Press, 1971), pp. 3–23.

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and shape from one person to another, produce a fundamental tone quality which can be varied as in singing and which together with characteristics of a person's vocal chambers (e.g., size of nasal cavity) determine the distinctive background voice qualities setting off one person's speech from everyone else's. Women usually have shorter vocal cords than men, so their tonal range is about an octave higher.

A guitar string played in open air gives a very thin sound. The sound is made fuller when it resonates through the large chamber of the guitar's body. Our three resonance chambers, the pharynx, mouth, and nose, add secondary vibrations to the basic sounds emitted by vibrating vocal cords. What comes out are the sounds of speech.

Size and shape of the oral cavity in particular can be varied tremendously. It can not only be narrowed and elongated, it can also be closed or partially closed by actions of the tongue and lips. Complete momentary closure of the mouth or pharyngeal cavities at any given place produces a class of sounds known as *stops* which can be either voiced *(b, d, g)* or voiceless (*p, t, k,* ʔ). If closure is not quite complete, the friction produces another class of sounds known as *fricatives (f, s, v, z)*. A stop and fricative produced together in rapid sequence results in an *affricate (e.g., č*—conventional notation for the *ch* in "child," *ǰ*—the *j* in "jack"). Stops, fricatives, and affricates represent different *manners of articulation*, but they are all *obstruents*, consonants that cannot function as syllable nuclei—the usual function of vowels.

Sounds that are not obstruents can be called *sonorants*. Sonorants carry the melody and accent in speech. This category of sounds can be subdivided into the *vowels (a, e, i, o, u), semivowels (w, y), liquids (l, r)*, and *nasals* (*m, n,* ŋ).

Consonants and semivowels can be classified by place of articulation in addition to manner and voicing. *Labials* include *p, b, f,* and *w* as well as several sounds unfamiliar to English speakers. *Dentals* include *t, d, s,* and *z* among others. *Palatals* include *č, ǰ, š, ž,* and *y*. *Velars* include *k, g,* and ŋ. *Uvular* sounds, rarely heard in America, are important in several languages including French and Kekchi. *Glottals* include the voiceless stop ʔ and the voiceless fricative *h*. Using the dimensions of voicing, manner of articulation, and point of articulation we can readily identify any consonant, as when we say that *b* is a voiced bilabial stop. Vowels are easier to deal with in terms of the dimensions tongue height and frontness–backness, as can be seen in the phonetic chart.

In addition to the above sound segments, there are *suprasegmental* qualities such as pitch and stress which are superimposed on syllable nuclei during speech. These can have language specific referential importance as when we use stress to differentiate "pérvert" (the noun) from "pervért" (the verb), or they can be primarily expressive in function.

We hear utterances that are really quite continuous, but we manage to divide them into discrete segments with relative ease. The elementary sound segments of a particular language are its *phonemes*, roughly corresponding to the letters with which we spell words. The word "pit," for example, can be analyzed into three distinct sound units, *p, i,* and *t*. We know that *p* and *b* represent two different phonemes in English

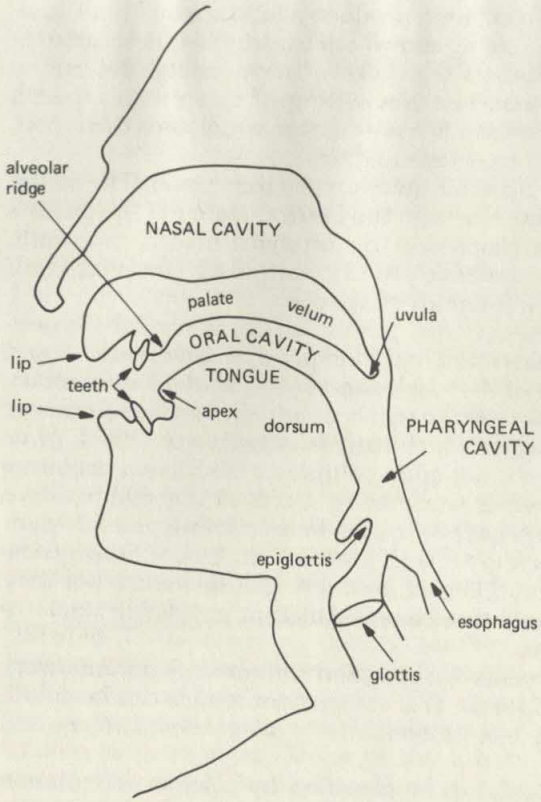

FIGURE 1. The Speech Organs.

because substituting *b* for the *p* in "pit" results in a word "bit" that has a different meaning. The phonemes *p* and *b* are meaningful units in English even though neither one can be assigned any particular meaning by itself. In some languages the sounds that we represent as *p* and *b* are simply different manifestations of a single phoneme. Substituting one for the other will not change the referential meaning of a word in which either appears.

Although we may feel that the *p* sound in "pit" is pronounced the same as the *p* sound in "spit," they are actually quite different. If you hold your hand a few inches in front of your mouth you can feel a puff of air, called *aspiration*, when you articulate the *p* in "pit." No such feeling attends the *p* in "spit," nor will you hear any aspiration. Clearly, the single English phoneme /*p*/ (written inside slashes to signal its phonemic status) has two different phonetic manifestations in the words "pit" and "spit." All variant pronunciations of the same essential sound, or phoneme, in a particular language are called *allophones* of the phoneme. Some variants have predictable—and complementary or mutually exclusive—distributions as in the example given, for in English *p*, *t*, and *k*

(voiceless stops) are normally aspirated if they begin a word. If, on the other hand, they follow an *s* in the same syllable, none of these stops will be aspirated. Such variation in the pronunciation of a phoneme is called *conditioned variation:* variation determined by some aspect of the environment in which an abstract entity—here a phoneme—is manifested. Variation for which conditioning environments are not specified, variation that cannot be predicted by rules, is *free variation.*

A moment's reflection may lead one to reject the above example of predictable distribution by imagining a situation in which "spit" could easily be articulated with an aspirated *p*, such as when the speaker is angry. Distribution is still predictable, but environmental conditioning must be more strictly specified to include expressive functions of speech in this case. In English aspiration is subphonemic—it doesn't distinguish one phoneme from another. That is why we can use it expressively (e.g., to express anger) by including it where it would not normally occur. This subject, departing from referential meaning, will be introduced again in the present chapter.

Phonemes, then, can be regarded as classes of related sounds that speakers of a particular language regard as a single sound. We have seen that aspirated *p* [p^h] and unaspirated *p* are treated by English speakers as the same sound. A Bolivian speaker of Quechua, however, would hear them as different sounds belonging to two different phonemes. Two words, identical except that one has an aspirated *p* and the other an unaspirated *p*, will mean different things in Quechua. Every language

	labial		dental		velar	uvular	glottal
STOPS							
voiceless	p		t		k	q	ʔ
voiced	b		d		g	G	
FRICATIVES							
Slit, vl.	ɸ	f	θ		x	h	
Slit, vd.	β	v	δ		γ		
Groove vl.			s	š			
Groove vd.			z	ž			
Lateral, vl.			ɬ				
AFFRICATES							
Groove, vl.			ʦ	č			
Groove, vd.				ǰ			
RESONANTS							
Nasal	m		n	ñ	ŋ		
Lateral			l				
Median	w		r	y			

	Front	Central	Back
High	i	ɨ	u
Lower High	ɪ		ʊ
Mid	e	ə	o
Lower Mid	E		ɔ
Low	æ	a	

FIGURE 2. Phonetic Chart.

divides up the universe of possible sounds differently: each possesses its own uniquely constituted set of phonemes.

While English makes use of thirty-three segmental phonemes, some languages have as few as twelve and others have as many as sixty. Because the phonemes of a language are patterned in a way that is unique to the individual language, and because these abstract units are related by rules to the actual sounds that we hear in ways that are language specific, we cannot directly compare phonemes across languages. This is a formidable problem if we want to make phonological generalizations about language.

The task of interlanguage sound system comparison can be accomplished, however, by decomposing phoneme segments not into classes of alternative sounds, but into bundles of simultaneous signalling units, or *features*, such as nasality, voicing, glottalization, and so on. Thus the phoneme /*p*/ in "pit" can be described as a combination of the three features labial, stop, and voiceless. These features alone are enough to distinguish it from any other English phoneme beginning a word, and can be called, therefore, the *distinctive features* of /*p*/. Although the *p* in "pit" is aspirated, the feature aspiration is a predictable manifestation of /*p*/ in this context and therefore nondistinctive: it is a redundant feature that is not needed to specify this phoneme.[3] Feature constituents of phonemes, rather than phonemes themselves, can be compared across languages.

By carefully selecting appropriate articulatory, auditory, and acoustic properties, it is possible to specify a pool of about fifteen features from which every language draws for its phonemic distinctions. A feature approach to interlanguage comparisons makes a great deal of sense. As we have seen, it would be meaningless to ask how many languages have the /*p*/ phoneme. It is possible, on the other hand, to ask how many languages utilize a particular feature (e.g., voicing) to create phonemic oppositions (e.g., between *p* and *b*), and how any given language might pattern its use of a particular feature.

Earlier I mentioned that some features are distinctive and some are redundant. This redundancy is an important part of language. Redundant features help us to identify the words that we hear because background noise and other sorts of communicative interference make necessary many secondary clues to help the listener in message interpretation. Redundancy in human language, in fact, remains right around the 50 percent level.

Another source of redundancy, apart from feature redundancy, derives from the fact that while our words are formed from combinations of phonemes, not every hypothetically possible phoneme sequence actually occurs in any language. In English, for example, no word will begin with a /*bd*/ or /ŋ/, nor does any word end with an /*h*/. Limitations on permissible sound sequences in a language can be described by general rules,

[3] To characterize the *p* in "spit" we need only specify the features labial and stop. Since voiced labial stops never occur after an *s* in English, to include the feature voiceless here would be redundant.

rules predicting redundant characteristics in the physical manifestation of speech forms. Predictable characteristics constitute one kind of redundancy. Repetition, as when we repeat a word for someone who didn't hear it the first time, is another kind of redundancy.

MORPHOLOGY

Words are the lifeblood of language, making the transition from sound to meaning possible. Each word can be thought of as representing a particular group of phonological and semantic properties—properties that are exchanged when we encode (speak) or decode (hear) linguistic messages. Each word also has syntactic properties. That is, it can combine with other words to form sentences, but only in rather restricted ways. For example, "sky" is a noun which can be combined with "blue," "the," and "is" in only two possible ways to result in a well-formed sentence: "The sky is blue" or "Is the sky blue?" Other permutations, such as "Sky the is blue," ignore the syntactic properties of these words.

Words participate in larger grammatical units like the phrase, clause, and sentence. They also can be broken down into smaller units carrying referential meaning, minimal units of this type being called *morphemes.* The word "cats," for example, consists of two morphemes, {cat} and the plural morpheme commonly represented as {Z}. These morphemes have a fixed order with respect to one another. To put it another way, words are internally stable combinations of morphemes.

Morphology is that aspect of grammatical description which deals with the internal structure of words, or—more narrowly—with rules by which morphemes are combined to form words. At first sight it may seem uncomplicated to formulate such rules. Couldn't we, for example, deal with nouns in English by saying that the plural morpheme is simply added to the end of the singular form as in "cats?" In most cases this rule with some further phonological specifications will work: but what about "deer," or "feet"? The former remains the same whether singular or plural, and the latter adds no plural marker to the end of the singular form, but depends instead on a vowel substitution to differentiate singular and plural. Static rules of arrangement are not sufficient, then, to relate the semantic properties of words to their phonological properties. It is necessary in some cases—while merely useful in others—to have rules of process which operate on some basic or underlying form, changing it to another in the appropriate contexts. Thus the underlying representation {foot} is manifested as "foot" when singular and as "feet" when plural, "foot" becoming "feet" by vowel change.

Linguists commonly distinguish *free morphemes,* forms that can stand alone as words (e.g., "cat," "go," "red"), from *bound morphemes* which have to be attached to other morphemes (e.g., "-ness," "-ing," "un-," or the plural morpheme {Z}). This division corresponds closely to the distinction between *roots* and *affixes,* roots being the basic forms which are modified by the addition of affixes to them. The roots of a language greatly outnumber the affixes, a useful point to remember.

Words, sometimes called *lexemes,* may be single free morphemes (e.g.,

"cat"), *compounds* of two or more free morphemes (e.g., "doghouse"), or free morphemes combined with one or more bound morphemes (e.g., "farmers"). Bound morphemes are affixed (prefixed, infixed, or suffixed) to the rest of a word. Some bound morphemes simply change words from one part of speech to another, as when "-ness" is suffixed to the adjective "sad" resulting in a noun "sadness." Others, such as the plural morpheme or the past tense morpheme, do not change the part of speech of the words involved. These two kinds of morphemes signal different types of information, the former typifying *derivational* affixes and the latter characteristic of *inflectional* affixes, which usually are attached only after the root has been modified already by any derivational affixes that it needs.

Some morphemes have several different phonological manifestations, called *allomorphs,* which may be determined by phonetic properties of surrounding morphemes. Let's recall the plural morpheme {Z} as an illustration. It is a bound morpheme that changes its shape to conform to phonetic properties of the words to which it is attached. Phonemically these variant shapes of {Z} are /*–s*/ (when added to such words as "cat," "lock," or "top"), /*–iz*/ (when added to such words as "church," "glass," or "bush"), and /*–z*/ (when added to such words as "dog," "car," or "pan"). If you listen closely to your own pronunciation of these words, you will see that there really are three different ways of pronouncing the same plural that we write with an *–s*. It is the sound directly preceding the plural ending that determines which of the three allomorphs will occur. In rule form, we could say that {Z} is manifested as /*iz*/ after /*s, z, š, ž, č,* and *ǰ*/, as /*s*/ after any other obstruent if it is voiceless, and as /*z*/ after any other voiced sound. As English speakers we follow this rule quite unconsciously, applying it even to nonsense words made up on the spur-of-the-moment. Rules of this sort, determining the phonemic shapes of morphemes on the basis of phonetic conditioning environments, are often termed *morphophonemic* rules.

There is another way of describing the formation of plurals in English (apart from the few exceptions like "oxen," "sheep," "mice," etc., which must be listed and learned separately). We can propose that the underlying phonemic representation of the plural is /*–z*/ which is acted on by two more general rules of English phonology to specify correct surface representations whenever the rules are applicable. The first rule roughly stated specifies that an /*i*/ must be inserted between any two obstruents sharing basically the same point and manner of articulation. From this rule we get /*kisiz*/ "kisses" from underlying /*kisz*/, for example. The second rule, which applies only after /*i*/ insertion, specifies that all consonants in a consonant cluster must be voiceless if the first one is voiceless. From this second rule we get /*lips*/ "lips" from underlying /*lipz*/, for example. These two rules apply quite generally in the sound system of English and help to account for the various manifestations of possessive and past tense inflection as well as plurals. The simplicity and productivity of this process-oriented approach is appealing, and it is beginning to replace the older arrangement-oriented approach illustrated just above it.

Languages differ in the means by which they express such concepts as plurality, negation, interrogation, tense, mood, shape, comparison, and so on. Of all the possible ways that a concept *could* be expressed, each language chooses some and ignores others. Moreover, several different means may be employed within a single language to express any given concept. As we have seen, plural formation in English takes place primarily in the morphological system through addition of a single bound morpheme suffix, although a few words are pluralized with different suffixes ("ox": "oxen"), internal vowel changes ("man": "men"), or no change at all ("deer": "deer"). Other existing languages express plurality with prefixes, infixes, tone changes, or separate free morphemes.

The fact that all known languages have some means for expressing plurality even though the means may differ substantially illustrates an important principle: that superficial differences often belie underlying similarities. The converse of this, that superficial similarities among languages often belie differences that become apparent only on closer inspection, is another point to remember. Tzeltal, an Indian language of Mexico, for example, seems to express plurality very much like English. For most nouns there is a single plural suffix /–etik/. A closer look, however, indicates that the "singular" (uninflected for plural) form of the noun actually means either singular or plural, depending on context, so that plural suffixes are frequently omitted even when the noun has a plural referent. In fact, the plural suffix is never used on nouns when preceded by numbers: *ča-tul ʔac'iš*, literally "two-person girl" means "two girls," for example. English speakers have to mark plural in count nouns referring to more than one thing, even when this is redundant as in "two girls." Tzeltal speakers shave off some of this redundancy because Tzeltal has *facultative* expression of the plural; they have a plural marker (a suffix), but they don't have to use it to convey the notion of plurality.

SYNTAX

Morphology is concerned with the internal structure of words, syntax with the internal structure of sentences. Both levels of organization involve the sequential arrangement of morphemes, so syntax can be construed to include morphology; and modern usage of the term syntax does just that. When we are dealing with a language such as English, in which a distinction between words and sentences can be made fairly easily, it may be useful to distinguish morphology from syntax. In other languages like Eskimo, where words very often are complete and complex sentences, the distinction is less useful.

To construct a sentence in any language we need to do two fundamental things, representing the syntactic processes *selection* and *arrangement*. First we must choose the words that best convey our thoughts. Then we have to arrange them in an appropriate order with the proper modifications—additions, deletions, and substitutions—so that they will conform to grammatical restrictions on sentence construction dictated by the language. To illustrate this point let's imagine that we witnessed a particularly surly canine biting the postman and wanted to tell someone about

it without giving any more information about the event than absolutely necessary, so we have selected only the words "bite," "dog," and "man." The job just can't be done in acceptable English with only these three words.

It would be ungrammatical to say "bite dog man" even though these alone are the vocabulary items originally selected to convey the intended message—translations of just these words in just this order would form a grammatical and appropriate sentence in many languages, but not English. A bit more sense can be made of the words when we arrange them as "dog bite man," but this is still not enough. The rules of English grammar force us to specify both nouns "dog" and "man" and to mark the verb "bite" for tense. Of several alternative possibilities, one solution would result in the grammatical and appropriate sentence "A dog bit the man." *Word order* here signals the grammatical relationships of subject and direct object (actor and acted-upon) as can be readily seen if we reverse the order of noun phrases with respect to the verb (i.e., "The man bit a dog"). Furthermore we have marked "dog" with the indefinite article by preceding it with "a," marked "man" with a preceding "the," and inflected "bite" for past tense in conformity with morphological rules of English. The point here is that syntactic rules of a language, like morphological ones, provide strong limitations on the spoken manifestations of messages we want to express.

Syntactic rules also give us tremendous power to create new, different, and complicated spoken messages. *Substitution* of entities within a sentence frame is one syntactic device which can function either to cut out needless repetition or to amplify the information content of any portion of the sentence. If, for example, the postman who was bitten above is already the subject of conversation between speaker and hearer, we can substitute the pronoun "him" for the noun phrase "the man," yielding "A dog bit him." Or, if we want to give more information about the dog involved, we might substitute "the big spotted dog that lives up the street" for "a dog," yielding "the big spotted dog that lives up the street bit the man." Note here that when we substituted "him" for "the man," the pronoun "he" took the form "him" in accordance with an English syntactic rule reflecting remnants of an older and more pervasive case system; and that the clause "that lives up the street" embedded in the second sentence is *subordinated* to "the big spotted dog," and is functioning here as an adjectival modifier of the noun phrase preceding it.

Subordination is actually a relationship holding between sentence constituents on many different levels. In the sentence, "I know that the postman likes small dogs," "small" is subordinated to "dogs" just as the subordinate clause "that the postman likes small dogs" is subordinated to the main clause "I know." Look again and you will see a whole sentence embedded in the subordinate clause, which shows how complex sentences can be derived by applying syntactic rules to combinations of simpler sentences.

Like rules for subordination, syntactic rules of *coordination* allow us to join two or more sentences together in several different ways. We

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could use the conjunction "and," for example, to put "John went to school" next to "Mary went to the store," forming the single sentence "John went to school and Mary went to the store." A *deletion* rule, one kind of substitution, could be applied here to cut out the repeated verb, which would give us "John went to school and Mary to the store." These two sentences differ slightly in their makeup, but they give essentially the same information and hence are *paraphrases* of one another.

Linguists (usually) judiciously use paraphrases to identify and characterize rules of sentence transformation—rules of deletion, insertion, and rearrangement—which are applied to sentence constituents in the process of changing an original sentence or combination of several sentences into the several possible paraphrases that can be derived. As we saw for process-oriented phonology and morphology, this involves postulating an underlying representation of abstract entities along with rules—some of them applying in a specific order with respect to one another—for arriving at the various possible surface representations which are paraphrases of each other. Many linguists today feel that transformational rules not only derive different paraphrases from the same underlying conceptual representation, but they introduce all of the morphological and syntactic trappings necessary to convert underlying conceptual representations into surface sentence manifestations. Among other things, rules of insertion, deletion, and rearrangement operate at various points in the derivation of a sentence to create words, to inflect for tense, number, and so on, to insert such dummy items as "it" in "It is raining," to make a sentence or smaller constituent negative, to change a statement into a question or a command, and to make an active sentence passive.

Two further syntactic processes should be mentioned here, special kinds of *co-occurrence restrictions.* They are *concord* (or agreement) and *government.* These forbidding words refer to quite simple processes involving the modification of words to conform to demands of other words co-occurring in the same sentence. Concord is merely a mutual dependency in form among words in a sentence. For example, "that" must be changed to "those" if it is qualifying a plural noun, as in "those men" or "those dogs"; if one stem requires the plural inflection, both do. Government is when the form of one word is determined by some syntactic characteristic of another, as in "the dog bit him" rather than "the dog bit he." English makes very little use of concord and government; other languages, like Latin, a great deal.

All languages convey information about things, events, and qualities, so it is not surprising that all have sentences in which we can isolate nounlike forms (nominals), verblike forms (verbals), adjectivelike forms (adjectivals), and adverblike forms (adverbials). Words belonging to these four classes are called *content words.* They convey more information than those belonging to other word classes. The other word classes, such as conjunctions, prepositions, determiners, and auxiliaries, give less information and often function primarily or exclusively to mark and specify relations among content words. These are called *function words.* Languages with few function words depend to a greater extent on affixes or root modification to signal these relations than do those with many.

Function words are important syntactic devices, then, some of which also carry semantic information. They are part of the glue which holds together the meanings of sentences.

Because you speak a language, you are capable of making up an infinite number of different sentences that can be readily understood by anyone speaking your language, even if the sentences have never been uttered. And you can do this with a vocabulary that may comprise no more than a few thousand morphemes. An incredible feat? Certainly, but all humans are capable of performing it. Rules for selection, substitution, arrangement, coordination, and subordination of sentence constituents—syntactic rules—make this possible.

Whereas morphemes, words, and idiomatic expressions have "dictionary meanings," syntactic rules contribute another sort of meaning to utterances which we can call "grammatical meaning," conveying such information as which sentence constituent is the indirect object or the subject, or which noun is being modified by an adjective. To put it another way, concepts and their interrelationships are expressed in sentences through operation of a linguistic code which allows us to attribute referential meaning to them. Language viewed as a referential system is responsible for communicating only a fraction of the total information that gets communicated when we speak.

PARALANGUAGE

Other communicative systems are superimposed on the linguistic one, augmenting or modifying the messages communicated by language in speech. One such system, overlapping the linguistic system, can be called *paralanguage.* Paralanguage includes all of the sounds coming from your mouth and nose that are not directly part of language. For the most part they accompany speech (e.g., drawling, whispering, shouting), but they can also convey messages in place of speech (e.g., shushing, wolf whistling, hissing). We can conveniently distinguish three separate subgroupings under the broad heading paralanguage. First there is the *voice set,* the physically and physiologically determined characteristics of an individual's voice that serve as a background for evaluating meaningful paralinguistic variations. One's voice set identifies him as a particular individual, and can give the listener information about his sex, health, age, body build, and even location.

Against a background of voice set, the listener measures two other aspects of paralanguage: vocal segregates and voice qualities. *Vocal segregates* include the many vocalizations we make that are expressive but not really a part of language. We can say *brrr* to indicate that it is cold, grunt "yes" or "no," make several varieties of tongue clicking for approval or disapproval, imitate a cock crowing, shush a noisy child, or hiss at a bad movie. These are all vocal segregates, and they can convey some rather specific messages.

Voice qualities include the loudness, pitch, and length variations superimposed on syllable nuclei that in some languages make referential distinctions. If they are distinctive referentially, then they are part of the

linguistic system. Where they do not play a part in the linguistic system, they are free to signal paralinguistic information. Other voice qualities that can accompany speech are whining, yawning, moaning, whispering, whimpering, giggling, laughing, and crying, as well as qualities interlocking these—tempo (from fast to slow), rhythm (from smooth to jerky), resonance (from thin to full-bodied), and others. Voice qualities can tell the listener if we are nervous, confident, angry, exuberant, irritated, or calm. That is, they are indicators of our emotional state among other things. Such information, naturally, is very important for the interpretation of vocal messages.

GESTURE

The man before you is very nervous. The side of his face is twitching. He keeps crossing and uncrossing his legs during the interview. His hands move jerkily as he points to a passage in the book. You note that he swallows and blinks frequently, and that his face is flushed. All are stereotypical signs of nervousness and are all conveyed through *gesture.* If we define the domain of gesture to include all visible body activity, we can divide it up in a way that neatly parallels paralanguage. First we must identify *gesture set* as the physical and physiological peculiarities of an individual's visible body activity which serve as a background for evaluating meaningful gestural variations. Like voice set, gesture set can give information about an individual's age, sex, health, and sanity.

Against a background of gesture set can be measured *gesture qualities* such as speed (from fast to slow), flow (from smooth to jerky), degree (from barely noticeable to pronounced), and others. These qualities can apply equally well to activities of the face, limbs, and posture. *Gestural segregates* such as a nod or shake of the head, a pointing finger, a goodbye wave of the hand, crossed legs, finger snapping, slapping the thigh, clapping, or a friendly wink have varying degrees of culturally determined meaning. They can be executed and described with reference to the gesture qualities noted above. Above all, they convey information that can amplify, emphasize, or modify linguistic and paralinguistic messages as well as information which can be transmitted in the absence of any vocal communication. As with the vocal-auditory mode of communication, the gestural-visual mode is always manifested in a social context, and it is the total context which provides the means necessary for interpretation of any message.

Gestural communication, often called *kinesics,* also has its analogue to language. That is, in some societies or social groupings within societies communication takes place through a gesture language. American sign language, for example, is a language for the deaf. It utilizes a vocabulary of gestural signs which combine in rule-governed ways to form gestural sentences. Possessing a vocabulary and syntax, a gesture language can be considered coordinate with, rather than necessarily subordinate to, a language proper. It performs similar functions, slightly differently expressed.

In addition to the vocal-auditory and visual-gestural channels of com-

I Vocal-Auditory (Hearing)
- A Voice Set
- B Voice Qualities
- C Vocal Segregates
- D Language

II Gestural-Visual (Sight)
- A Gesture Set
- B Gesture Qualities
- C Gestural Segregates
- D Gesture Language

III Touch

IV Taste

V Smell

FIGURE 3. Modes of Communication.

munication, we also transmit some information that is perceived through the senses of smell, taste, and touch. Most obvious are such things as kissing, spanking, wearing perfume, shaking hands, and hugging. You can probably think of many more to add to these. One day, perhaps, these communicative modes will be studied as closely as vocal and gestural communication have been.

SEMANTICS

In a nutshell, semantics is the study of meaning. The nature of meaning and the way that it should be represented in a linguistic model, or grammar, is the most crucial and disputed question in current linguistic theory. Up to this point I have taken pains to restrict linguistic meaning to referential meaning only. At this level the "I'm hungry" uttered by a starving beggar has the same meaning as the "I'm hungry" said by a child to his mother at bedtime when he wants to stay up a little longer.[4] The linguist has until recently been content to treat these utterances as identical with respect to meaning, and has referred to the sort of meaning shared by them as "core meaning," "literal meaning," "dictionary meaning," "primary meaning," or "referential meaning." Linguistic models predicated on this kind of meaning disregard the fact that these two utterances may differ in voice qualities of the speakers, desired goals of the speakers, intonational patterns of the speakers, gestural accompaniments to the utterances, cultural knowledge and presumptions of the

[4] Leonard Bloomfield, *Language* (New York: Holt, Rinehart and Winston, 1933), pp. 141–142.

speakers and hearers, inferences made by the hearers, and appropriateness of the utterances in context among other things. In short, they must disregard vast quantities of information transmitted by the two speech acts which are not common to both.

On the other side of the coin, we can think of situations in which "Shut the door," "It's noisy in here," "Would you be so kind," or simply a pointed finger could all be considered functionally equivalent requests, sharing the same pragmatic meaning. Again, linguistic models of the sort mentioned above have to treat each of them differently, ignoring the level of meaning that they share.

As meaning has become a focal question for linguistics in the last few years, some theoretical linguists have come to see language as a system that cannot be clearly analyzed apart from other cultural systems of communication, apart from the social contexts in which it is manifested, apart from the social meanings that figure prominantly in determining some aspects of sentence structure. These linguists have found it necessary in making judgments of grammaticality and in defining constraints on co-occurrence of sentence constituents to augment the core referential meaning traditionally associated with linguistic form by considering other meaningful characteristics of utterances. Utterances, or sentences, are then seen to encode not only core meanings but also *entailed propositions* (e.g., "John's baldness caused him much worry" entails the proposition "John was bald"); *presupposed propositions* (e.g., "Mary was bald" must be interpreted in the light of such presuppositions as that "Mary probably hides it with a wig," "Women are more rarely bald than men," and "Mary is probably a woman because Mary is a woman's name"); *implied propositions* (e.g., "John wasn't really happy until yesterday" can imply that "John has been really happy since yesterday"); and *illocutionary propositions* (e.g., "Shut the door" includes the unstated illocutionary proposition "I am commanding you" as a necessary part of the sentence).

Language as a system, abstracted from social situations, limited to neutral or normal referential meanings of utterances, and restricted to inferred grammatical rules pertaining to an idealized speaker-hearer in a homogeneous speech community, is a legitimate concern of structural linguistics. Somewhere along the way, however, speech in context inclusive of variation slipped in the back door of linguistic study and has become the central concern of sociolinguistics.

SOCIOLINGUISTICS

Language seen in terms of a single referential function has for many years been a useful way to separate and intensively study a communicative system that can be to some extent distinguished analytically from other systems of human communication with which it articulates. The fact is, however, that language is typically manifested in situated speech events, in social contexts within which it is often difficult to distinguish linguistic elements from nonlinguistic ones. Many communicative functions are manifested in the same speech act; many kinds of information are conveyed from one person to another. Hence, the spoken utterance in context

weaves social and grammatical knowledge together, often inseparably so.

Looking at it another way, the child who is acquiring language is simultaneously learning his society's rules for when to speak and when not to; when to use respect forms, baby talk, slang, or a secret language; how to open and close conversations; how to announce, apologize, congratulate, or explain in a culturally approved manner; when to quote, and numerous other communicative skills—sociolinguistic skills that so clearly go hand in hand with the acquisition of language. A child who grew up capable of generating grammatically correct utterances but who couldn't create and choose appropriate ones at the right time would certainly be a monster.

Each of us, then, has our own repertoire of sociolinguistic knowledge that we draw upon for better or for worse to say what we have to say. And what we have to say is interpreted or misinterpreted on the basis of our listener's repertoire of sociolinguistic knowledge. To the extent that individual repertoires have shared features or can be made to mesh through continuing establishment of common understandings, communication is effective.

Effective communication within a social group underlies the basic sociolinguistic concept of *speech community*, "a community sharing knowledge of rules for the conduct and interpretation of speech."[5] Within a speech community one can isolate many situations in which specifiable rules governing the use of speech characterize particular *speech events*. A speech event can be composed of one or more *speech acts*, these being minimal communicative acts employing verbal means, which can occur in one or more situational contexts. Dell Hymes, an American pioneer in the field of sociolinguistics who is responsible for these terms, notes as illustration that in our society a joke can occur in such varied contexts as a lecture, a formal introduction, or a sermon; "joke" is the label that we give to this speech act. A joke (speech act) may also occur within a conversation (speech event) that takes place during a party (speech situation). Every society has its own criteria for defining and naming speech acts, speech events, and speech situations; and every society has its own values with respect to the importance of various components and functions of speech acts.

A speech act can be dissected in terms of seven essential components: (1) sender; (2) receiver; (3) message form; (4) message channel; (5) topic; (6) code; (7) setting.[6] Nine broad functions, any of which may or may not be emphasized in any particular speech act, are potentially applicable

[5] Dell Hymes, *Foundations in Sociolinguistics* (Philadelphia: University of Pennsylvania Press, 1974), p. 51. Subsequent definitions of sociolinguistic concepts are also adapted from Hymes.

[6] These components are set forth with more illustrative detail in Dell Hymes, "The Ethnography of Speaking," in *Anthropology and Human Behavior*, eds. T. Gladwin and W. C. Sturtevant (Washington, D.C.: Anthropological Society of Washington, 1962), pp. 13–53. They have been modified and augmented by several additional speech act components in subsequent articles by Hymes.

to the speech act as a whole. Each function can be attributed to a primary association with one of the seven speech act components.

The *sender* (or speaker) need not always be a human. There are societies in which a musical instrument, certain animals, or even a clap of thunder may be the sender in a speech act. Sounds produced by the sender can function to tell the receiver something about the speaker's age, sex, area of origin, and often other personal features; his voice set, intonation, rhythm, speed, pronunciation, vocabulary, and style serve, in short, to identify him. This is the *identification function.*

Choice of words, intonation, relative speed of response, and other aspects of the speaker's vocal performance may also function expressively, to express emotions and attitudes towards the receiver or any other component of the speech act. The *expressive function* is particularly apparent when it operates in opposition to the referential meaning of what has been said. Consider, for example, the child who tries to avoid a scolding by saying, "I only said that you're a real nice person, Mommy." But her mother isn't fooled and replies, "It's not the words (referential function) that bother me, but the way (expressive function) that you said them."

Potential *channels* of communication in various societies include gestures, whistling, and drumming, as well as speech. Principally associated with message channel is the *contact function,* whether physical in the sense of sound waves hitting the receiver's ear, or psychological in the sense of speaking just to be speaking. Communication for its own sake is an important part of social life and is found more highly developed in some people than in others. Perhaps you have noticed that often in elevators people speak simply to maintain psychological contact. Silence in our own culture, when prolonged, could in some circumstances be interpreted as implied hostility. Pleasant but noninformative verbal rituals, as on meeting a friend or talking to a stranger in an elevator, serve to allay anxiety; the receiver thereby learns something of the sender's intentions.

The *form* of a message can be said to have a *poetic function.* Though not limited to poetry, the poetic function is expressed as restrictions on message form. There are different degrees and varieties of aesthetic pleasure derivable from various ways of formulating a message with any given referential content.

The *referential function* in speech is most directly associated with the *topic* of a speech act, and is closely tied to the "dictionary meanings" of messages. When we take something literally, we are appealing to the referential function, which is the key function for structural description of language.

The *code* component of a speech act refers to the signalling units of which messages are composed and to the rules for combining these units. When we speak of phonemes, morphemes, and sentences, we refer to elements of the linguistic code. Similarly we can think of technical jargon, slang, formal speech, and other levels and varieties of speech, as well as dialects and languages, as codes or combinations of several codes. The *metalinguistic function* is associated primarily with the code(s) involved.

Information about the code which is conveyed in a speech act is metalinguistic information. We consciously manifest an interest in the metalinguistic function when we ask questions like "What language is that?" "How do you spell it?" or "Are you sure that's not just slang?"

When we focus attention on the *receiver* in a speech act, two general functions can be involved: one directive and the other rhetorical. The *directive function* concerns subsequent activity of the receiver as directed by what the speaker says. The *rhetorical function* concerns the receiver's outlook as it is affected by what is said. These two functions correspond to overt behavior on the one hand and to attitudes and beliefs on the other. They are interrelated in that subsequent activity is partly determined by attitude or outlook.

The *setting* component is defined in terms of criteria that may involve all six of the other components. Relevant features which constitute a particular setting most often involve participants, location, and time of the speech act. The *contextual function* associated with the setting component is reflected in messages that tell something about the time, place, or persons in the interaction. Many linguistic forms referring to these things cannot be interpreted without reference to the speech act itself, for their meanings are not fixed but relative (e.g., "me," "you," "here," "there," "now," "then").

Try replacing the word "function" in the above paragraphs with the word "meaning." This will give you some idea of the variety of meanings or types of information that are recoverable from speech. Any component of a speech act, it should be emphasized, may contribute any of the kinds of functional meanings discussed. The functions have been assigned here to individual components on the basis of dominant associations. Whereas traditional conceptions of the linguistic system focus on the referential function, the sociolinguistic system must be seen in terms of all nine functions; nine different types of meaning are involved.

In the wider context of sociolinguistic meaning we can say that aspiration or vowel length in English, for example, make meaningful distinctions. These features are nondistinctive in English phonology with respect

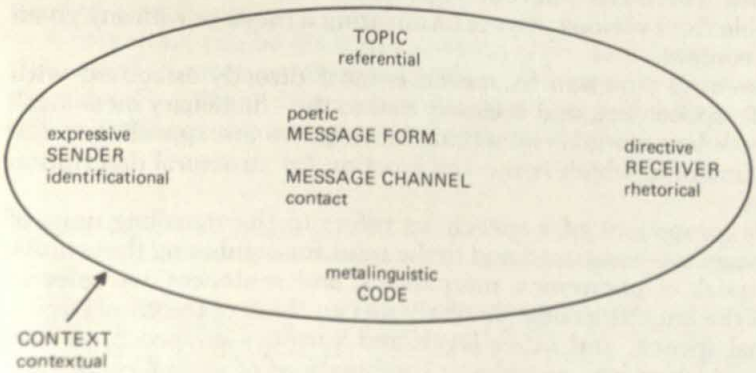

FIGURE 4. Components and Functions of the Speech Act.

to the referential function, but they can clearly be distinctive in terms of expressive function, signalling different attitudes on the part of the speaker.

Sociolinguistic description with its broadened framework of speech-act components and functions has helped to revive interest in larger linguistic constructions than sentences. When we speak, for example, the internal structure of one sentence often determines the internal structure of others in the same *discourse unit* (e.g., the paragraph). Syntactic rules apply, in other words, not just to sentences and their constituents, but also to combinations of sentences.

Syntactic and phonological rules can be grouped under three distinct headings corresponding to different relationships among units. There are *rules of alternation* where a unit on one level of abstraction is represented by several on another. Phonemes are represented as allophones on a more superficial level, for example. The relationship implied here is specified by our conjunction "or." *Rules of co-occurrence* tell us which units occur in simultaneous combination with others to represent a single unit on another level of abstraction. Phonemes, for example, can also be represented as combinations of features. The relationship implied here is specified by our conjunction "and." Finally, there are *rules of sequencing* (or concatenation) which tell us how elements can be placed one after another in the formation of larger units on the same level of abstraction. Another example from phonology would be the rules arranging phonemes in a syllable. Sociolinguistic rules, it turns out, also fall into these three categories of rules.[7]

SUMMARY

Language is man's most important means of transferring ideas from one mind to another. Of all our senses, hearing best meets the demands of human interaction for receiving referential messages. Speech can take place in the dark and with the hands full. In order to make use of the hearing sense, ideas must somehow be converted to sound. Our speech organs produce the sounds into which ideas have been converted in patterned ways. This jump from idea to speech sound is based on man's ability to let one thing stand for another—called symbolization—and involves a complex organization of processes that is often referred to as linguistic structure. If we wish to consider language as only a referential system, then we can use the notion of sociolinguistic system or communicative repertoire to take into account other functions of speech. Because social factors are usually inextricably woven into linguistic constructions, we may end up having to broaden our conception of language to include what is presently seen as the sociolinguistic system.

[7] An absorbing treatment of sociolinguistics that isolates these rule types in a slightly different way is given by Susan Ervin-Tripp, "Sociolinguistics," in *Advances in Experimental Social Psychology*, vol. 4, ed. L. Berkowitz (New York: Academic Press, Inc., 1969), pp. 91–165.

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2

Theories of Culture

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INTRODUCTION

"Yanomamö culture," "Japanese culture," "the evolution of culture," "nature vs. culture": we anthropologists are still using that word, and we still think it means something. But looking across at our primate relatives learning local traditions, using tools, and manipulating symbols, we can no longer say comfortably that "culture" is the heritage of learned symbolic behavior that makes humans human. And standing amid the swirling tides of change and individual diversity, we can no longer say comfortably that "a culture" is the heritage people in a particular society share. Moreover, we increasingly realize that the holistic, humanistic view of culture synthesized by Kroeber and Kluckhohn includes too much and is too diffuse either to separate analytically the twisted threads of human experience or to interpret the designs into which they are woven.

[1] I am indebted to the Center for Advanced Study in the Behavioral Sciences, Stanford, California, for providing an idyllic setting where this review could be written, and to Bridget O'Laughlin, Mervyn Meggitt, Triloki Nath Pandey, and Gregory Bateson for their helpful suggestions.

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The challenge in recent years has been to narrow the concept of "culture" so that it includes less and reveals more. As Geertz argues, "cutting the culture concept down to size . . . [into] a narrowed, specialized, and . . . theoretically more powerful concept" (30, p. 4) has been a major theme in modern anthropological theorizing.[2] And predictably, modern anthropologists have not agreed on the best way to narrow and sharpen the central conceptual tool they have inherited from their elders.

In the pages that follow, I will summarize recent rethinkings of "culture" as falling into four focal areas. Having highlighted work being done in each area,[3] I will seek to clarify the terminological, philosophical, and substantive issues that divide major theorists. In the process, I will consider the implications of these rethinkings for a number of classic anthropological questions: How have cultures developed and what forces shape them? How are cultures learned? How do shared symbolic systems transcend individual thought worlds? How different and unique are cultures? Do universal patterns underlie diversity? How is cultural description to be possible?

CULTURES AS ADAPTIVE SYSTEMS

An important expansion of cultural theory has come from viewing cultures in evolutionary perspective. A widened bridge between studies of hominid evolution and studies of human social life has led us to see more clearly that the human biological design is open-ended, and to perceive the way its completion and modification through cultural learning make human life viable in particular ecological settings. Applying an evolutionary model of natural selection to cultural constructions on biological foundations has led anthropologists to ask with increasing sophistication how human communities develop particular cultural patterns.

A vast literature, popular and technical, has dealt with the interweaving and relative importance of biological and cultural components of human behavior. Aggression, territoriality, sex roles, facial expression, sexuality, and other domains where cultural and biological are interwoven have been endlessly and often mindlessly discussed. From all this, we shall draw two brief conclusions, then pass on. First, any notion that if we peel off the layers of cultural convention we will ultimately find Primal Man and naked human nature underneath is both sterile and dangerous: we need a complex interactional model, not a simplistically stratigraphic one (19, 25). Second, either extreme ethological or extreme cultural determinism can now be sustained by ideology and faith but not by sober

[2] Implicit in this view is an assumption shared by most of us, I think, that "culture" does not have some true and sacred and eternal meaning we are trying to discover; but that like other symbols, it means whatever we use it to mean; and that, as with other analytical concepts, human users must carve out—and try to partly agree on—a class of natural phenomena it can most strategically label.

[3] I will *not* list exhaustively the publications where "culture" is used or cultural theory is applied or developed. Since that would include a substantial proportion of the writings in anthropology, this would not only be impossible, but trivial and unrevealing: a focus on high points and highlights is clearly demanded in a review article concerned with refinement of theory, not accumulation of substance.

science. Just how biological templates are transformed and elaborated into cultural patterns will have to be worked out for each realm; and that will take careful and imaginative research designs and patient exploration, not polemics and sensationalism.

How human cultures are distinctive, despite the continuities in hominid evolution, has been extensively reviewed by Holloway (45), Alland (2, 3), Montagu (59), and others. A crucial issue here is how and at what stage vocal language evolved and what its immediate precursors were (44). If the evidence that an elaborated vocal language is less than 100,000 years old holds up, a vast period looms when early humans lived in bands, made tools, hunted in well-planned forays, probably lived in pair-bond family relationships—a period of two million years or more of proto-human social life without a fully elaborated code for symbolic communication. Our understanding of what makes humans human and how cultures evolved will doubtless unfold and change excitingly in the next few years.

From the standpoint of cultural theory, however, the major developments have come from evolutionary/ecological approaches to cultures as adaptive systems. The major spawning grounds of evolutionary/ecological rethinkings have been Michigan and Columbia. The foundations laid by Leslie White have been creatively recast by such scholars as Sahlins, Rappaport, Vayda, Harris, Carneiro; and by such theory-minded archeologists as the Binfords, Flannery, Longacre, Sanders, Price, and Meggers. The rapprochement of a theoretical archeology with ecological anthropology emerges as one of the major developments of the past decade.

That is not to imply that consensus prevails about how cultures are best conceptualized or how and why they develop and change. The recent exchanges between Service (75) and Harris (42), Marxist critiques of Harris's cultural materialism, the gulfs between cultural ecology and the human ecology conceived by Vayda & Rappaport (81), the sectarian wars within "the new archeology," all attest to diversity and disagreement. Given this sectarian diversity, most scholars working in this tradition (I will for shorthand call them "cultural adaptationists")[4] agree on some broad assumptions.

(a) Cultures are systems (of socially transmitted behavior patterns) that serve to relate human communities to their ecological settings. These ways-of-life-of-communities include technologies and modes of economic organization, settlement patterns, modes of social grouping and political organization, religious beliefs and practices, and so on. When cultures are viewed broadly as behavior systems characteristic of populations, extending and permuting somatic givens, whether we consider them to be patterns *of* or patterns *for* behavior is a secondary question.

> Culture is all those means whose forms are not under direct genetic control . . . which serve to adjust individuals and groups within their ecological communities (Binford 11, p. 323).

[4] A term which, however disagreeable, lacks the aura of ancient battles, rusting weapons, and buried protagonists that "cultural evolutionists" conjures to mind.

> The culture concept comes down to behavior patterns associated with particular groups of peoples, that is to "customs" or to a people's "way of life" (Harris 41, p. 16).

(b) Cultural change is primarily a process of adaptation and what amounts to natural selection.

> Man is an animal and, like all other animals, must maintain an adaptive relationship with his surroundings in order to survive. Although he achieves this adaptation principally through the medium of culture, the process is guided by the same rules of natural selection that govern biological adaptation (Meggers 56, p. 4).

Seen as adaptive systems, cultures change in the direction of equilibrium within ecosystems; but when balances are upset by environmental, demographic, technological, or other systemic changes, further adjustive changes ramify through the cultural system. Feedback mechanisms in cultural systems may thus operate both negatively (toward self-correction and equilibrium) and positively (toward disequilibrium and directional change).

(c) Technology, subsistence economy, and elements of social organization directly tied to production are the most adaptively central realms of culture. It is in these realms that adaptive changes usually begin and from which they usually ramify. However, different conceptions of how this process operates separate the "cultural materialism" of Harris from the social dialectics of more authentic Marxists or the "cultural evolutionism" of Service and distinguish the cultural ecologists of the Steward tradition from human ecologists such as Rappaport and Vayda. However, all (except perhaps the Rappaport of most recent vintage) would view economies and their social correlates as in some sense primary, and ideational systems—religion, ritual, world view—as in some sense secondary, derived, or epiphenomenal.

Service's charges of monistic reductionism are misplaced here (see 42, 75). Harris's analytical strategy expresses an expectation, not an assumption:

> Similar technologies applied to similar environments tend to produce similar arrangements of labor in production and distribution, and . . . these in turn call forth similar kinds of social groupings, which justify and coordinate their activities by means of similar systems of values and beliefs (41, p. 4).

In assigning "priority to the study of the material conditions of sociocultural life," Harris—like other articulate proponents of related views—does not invoke a simple "prime-mover," but a complex of them (he himself speaks of "demo-techno-econo-environmental conditions"); and he and other cultural adaptationists leave room for cases where an ideology, home grown or imported, transforms the social and economic order. Marxist critics of Harris also stress—rightly, I think—the importance of conflicts and contradictions in the social order, not simply adaptation, in generating and guiding processes of social and cultural change.

(d) The ideational components of cultural systems may have adaptive consequences—in controlling population, contributing to subsistence, maintaining the ecosystem, etc; and these, though often subtle, must be carefully traced out wherever they lead:

> . . . It is necessary to consider the total culture when analyzing adaptation. Superficially, it might be assumed that attention could be confined to aspects directly related to the environment . . . [But] whether analysis begins with religious practices, social organization, or some other sector of a cultural complex, . . . [it] will . . . reveal functional relationships with other categories of behavior that are adaptive (Meggers 56, p. 43).

The most striking recent elaboration of this view has been Rappaport's impressive analysis of Tsembaga Maring ritual cycles as components in an adaptive system (65); and more recently, his suggestion that ritual systems and the cultural frame of sanctity play a central part in mediating cultural adaptation (66–68).

IDEATIONAL THEORIES OF CULTURE

In contrast to the diverse adaptationist theorists of culture stand a number of theorists who see cultures as ideational systems. Here I will distinguish three rather different ways of approaching cultures as systems of ideas.

Cultures as Cognitive Systems

Another major theme of the last 15 years has been the emergence of an explicit cognitive anthropology. In practice, "the new ethnography" has been mainly an exploration of systems of folk classification ("ethnoscience," "ethnographic semantics"). But beyond the anthropological study of other peoples' butterfly collecting has emerged a new and important view of culture as cognition.

Cultures are seen as systems of knowledge. To quote Ward Goodenough:

> A society's culture consists of whatever it is one has to know or believe in order to operate in a manner acceptable to its members. Culture is not a material phenomenon; it does not consist of things, people, behavior, or emotions. It is rather an organization of these things. It is the form of things that people have in mind, their models for perceiving, relating, and otherwise interpreting them (32, p. 167).

> Culture . . . consists of standards for deciding what is, . . . for deciding what can be, . . . for deciding what one feels about it, . . . for deciding what to do about it, and . . . for deciding how to go about doing it (33, p. 522).

Goodenough contrasts this ideational sense of culture with the sense used by the adaptationists we have discussed, who conceive culture to be the "pattern of life within a community—the regularly recurring activities and material and social arrangements" (33, p. 521; 34–37).

So reconceived, cultures are epistemologically in the same realm as language (Saussure's *langue* or Chomsky's competence), as inferred ideational codes lying behind the realm of observable events.

In this conceptualization, language is a subsystem of culture; and explorers in cognitive anthropology have hoped or assumed that linguistic methods and models will be appropriate to other cultural realms: hence componential analysis, emic vs. etic, eliciting frames, etc. [See Keesing's argument (48) that cognitive anthropologists have made this jump too lightly, and have borrowed from a now outmoded taxonomic linguistics.] But in the last several years, attention has begun to turn from the uniqueness of cultural systems to a search for universal patterns (48).

Analyses of cultures as cognitive systems have not progressed very far beyond a mapping of limited and neatly bounded semantic domains. Significant attempts to formalize the cultural knowledge needed to stage performances or operate in limited social situations have been made by Frake (18), Metzger & Williams (57), Wallace (83), Spradley (77), Agar (1), and others; but it is striking in retrospect that the messianic optimism of early cognitive anthropology has yielded so few fragments of cultural description.

Moreover, it has yielded few even tentative sketch maps of the overall structure and organization of cultures as cognitive systems (see e.g., 50, p. 123; 34, pp. 258–59; 37; 78). Not only has the notion of a "cultural grammar" proved unproductive and inadequate in the face of the staggering richness and complexity of human knowledge and experience; "new ethnographers" have not set out even an empty blueprint of how an overall cognitive system might be organized, and hence how the bits and pieces offered in demonstration might fit into a wider design. Such a lack of broad vision, I believe, has obscured the magnitude of the realms of culture not amenable to the surface probings of formal ethnography. I have argued (48, 49) that the new transformational linguistics gives some valuable insights about how cultural knowledge underlying the surface structures so far mapped might be organized; below I will argue that burgeoning research in artificial intelligence can yield further insights.

Cultures as Structural Systems

On the continent, Lévi-Strauss has continued to elaborate his view of men's symbolic worlds and the processes of mind that generate them; and in the last decade, structuralist approaches have had profound impact on many scholars trained in the Anglo-American tradition.

Lévi-Strauss's writings on culture and mind have not only been sweepingly influential; as sacred texts, they have elicited an ever-widening stream of exegetical literature.[5] I will not add substantially to that stream. Here only a few points will serve to place the Lévi-Straussian position in relation to those that precede and follow. Lévi-Strauss views cultures as shared symbolic systems that are cumulative *creations of mind;* he seeks to discover in the structuring of cultural domains—myth, art, kin-

[5] Literary critics have a tendency to be ponderous, obscure, and intellectually pretentious, in counterpoint to the textured beauty of the texts they seek to illuminate; and Lévi-Strauss's exegetes and apologists have with rare exceptions (notably Boon 12) carried on this tradition.

ship, language—the principles of mind that generate these cultural elaborations. Material conditions of subsistence and economy constrain (but do not *explain*) lived-in worlds; but especially in myth, they leave thought-of worlds free reign. The physical world humans live in provides the raw materials universal processes of mind elaborate into substantively diverse but formally similar patterns. The mind imposes culturally patterned order, a logic of binary contrast, of relations and transformations, on a continuously changing and often random world. The gulf between the cultural realm, where man imposes his arbitrary order, and the realm of nature becomes a major axis of symbolic polarity: "nature vs. culture" is a fundamental conceptual opposition in many—perhaps all—times and places. Lévi-Strauss, especially in *Mythologiques*, is more concerned with "Culture" than with "a culture": he sees American Indian mythic structures as overlapping, interconnected patterns that transcend not only the cognitive organization of individual Bororo or Winnebago or Mandan actors, but in a sense transcend as well the boundaries of language and custom that divide different peoples.

Cultures as Symbolic Systems

Another avenue of approach to culture, related to but distinct from both the American cognitivist and continental structuralist approaches, has been to treat cultures as systems of shared symbols and meanings (13). On the continent, this avenue has been most extensively explored by Louis Dumont.[6] In the United States, the most notable pioneers have been two anthropological heirs to the Parsonian tradition: Clifford Geertz and David Schneider.

Geertz's powerful view of culture, illumined by a broad humanistic scholarship, has become increasingly systematic. Like Lévi-Strauss, Geertz is at his best when he draws on general theory to interpret ethnographic particulars; unlike Lévi-Strauss, he finds these particularities in the richness of real people in real life: a cockfight, a funeral, a sheep theft. His texts are not disembodied and decontextualized myths or customs, but humans engaging in symbolic action.

Geertz sees the cognitive view of Goodenough and the "new ethnographers" as reductionistic and spuriously formalistic. Meanings are not "in people's heads"; symbols and meanings are shared by social actors—between, not in them; they are public, not private.[7] Cultural systems are ideational, but in the sense that a Beethoven quartet is ideational—beyond or between its manifestations in individual minds or concrete performances. Cultural patterns, he says, are not reified or metaphysical: like rocks and dreams "they are things of this world."

Geertz sees his view of culture as *semiotic*. To study culture is to study shared codes of meaning. Borrowing from Ricoeur a broader sense of "text," Geertz recently has treated a culture as "an assemblage of texts" (29, p. 26; cf. 13). Anthropology thus becomes a matter of *interpretation*, not decipherment (in this, Geertz contrasts his own approach with Lévi-

[6] Dumont's important ideas will not, due to limitations of space, be reviewed here.
[7] In this Geertz draws on Husserl, Wittgenstein, and Ryle.

Strauss's (see Geertz 28 and 29, p. 36, f.n. 38);[8] and interpretation becomes "thick description" that must be deeply embedded in the contextual richness of social life.

Geertz has no ethnoscience optimism that the cultural code can be formalized as a grammar, no Lévi-Straussian glibness at decoding: interpreting cultural texts is a slow and difficult task. How a culture as an assemblage of texts fits together is nowhere made clear (presumably, Geertz would agree that we are still in the early stages of finding that out). When he has stepped back to generalize about religion, ideology, and common sense as cultural systems and about Balinese concepts of time and person (24, 26, 27, 30, 31), some picture of the relation between cultural domains begins to emerge. His view of the wider patterning of culture emerges most vividly in an extension of Wittgenstein's analogy between our language and an old city, "a maze of little streets and squares" that are the precipitate of time, surrounded by a neat reticulate design of planned modern sections—the formal languages of mathematics and science. Cultures are, Geertz argues, like old cities. The ones anthropologists usually study, unlike our own, have few if any planned suburbs (and that, he argues, makes somewhat spurious the anthropological effort to find in the wandering streets of those ideational cities the sectors that correspond to our neatly planned suburbs of philosophy, law, or science). The analogy is vivid; and Geertz has made a notable effort to explore some sectors of several old and tangled cities, to convey the subtle spirit of the streets as well as their rough map, and to generalize about the corresponding sectors of different cities. The overall plan of these cultural cities cannot yet be seen. Elsewhere, Geertz warns against the danger of the analyst mapping a culture in such a way as to maximize and neaten its integration and internal consistency—where in fact only partial integration and often disconnectedness and internal contradiction exist. He creates another vivid metaphor:

> . . . The problem of cultural analysis is as much a matter of determining independencies as interconnection, gulfs as well as bridges. The appropriate image, if one must have images, of cultural organization, is neither the spider web nor the pile of sand. It is rather more the octopus, whose tentacles are in large part separately integrated, neurally quite poorly connected with one another and with what in the octopus passes for a brain, and yet who nonetheless manages to get around and to preserve himself, for a while anyway, as a viable, if somewhat ungainly entity (27, pp. 66–67).

A related but somewhat different tack has been taken by David Schneider. Like Geertz, Schneider began from a Parsonian framework, but he, too, has developed it in a distinctive way (drawing heavily on the insights of Dumont).

Schneider's view of culture is clearly expressed in his introduction to *American Kinship: A Cultural Account.* Culture, he tells us, is a system

[8] Note a further central contrast between Lévi-Strauss and Geertz, especially as drawn by the former: Lévi-Strauss's rejection [as argued in *L'Homme Nu* (54)] of the subjectivist orientation of phenomenology, which Geertz follows in drawing on Schutz and on the Parsonian actor frame of reference.

of symbols and meanings. It comprises categories or "units," and "rules" about relationships and modes of behavior. The epistemological status of cultural units or "things" does not depend on their observability; both ghosts and dead people are cultural categories. Nor are rules and categories to be inferred directly from behavior; they exist, as it were, on a separate plane. "The definition of the units and the rules is *not* based on, defined by, drawn from, constructed in accord with, or developed in terms of the observations of behavior in any direct, simple sense" (71, p. 6). And, as Schneider's analysis of kinship makes clear, he believes that analysis of cultures as systems of symbols can profitably be carried out independently of the "actual states of affairs" one can observe as events and behaviors. There are, he admits, important questions to be asked about the connections between the plane of cultural symbols and the plane of observable events so that one can "discover how the cultural constructs are generated, the laws governing their change, and in just what ways they are systematically, related to the actual states of affairs of life" (71, p. 7); but in his recent work, he has chosen to leave those tasks to others.

More recently, Schneider (72), has expanded and clarified his conception of culture. He distinguishes a level of "how-to-do-it" rules or *norms* that tell an actor how to navigate in his social world. But he wants in cultural analysis to take one further step back, to distinguish "the system of symbols and meanings embedded in the normative system, but . . . a distinct aspect of it [which] . . . can easily be abstracted from it":

> By symbols and meanings I mean the basic premises which a culture posits for life: what its units consist in; how those units are defined and differentiated; how they form an integrated order or classification; how the world is structured; in what parts it consists and on what premises it is conceived to exist, the categories and classifications of the various domains of the world of man and how they relate one with another, and the world that man sees himself living in (72, p. 38).

Since Schneider's contrast between "normative" and "cultural" levels is conceptually important, it is worth quoting him at greater length as he clarifies it:

> Where the normative system . . . is Ego centered and particularly appropriate to decision-making or interaction models of analysis, culture is system-centered . . . Culture takes man's position vis-à-vis the world rather than *a* man's position on how to get along in this world as it is given. . . . Culture concerns the stage, the stage setting, and the cast of characters; the normative system consists in the stage directions for the actors and how the actors should play their parts on the stage that is so set (72, p. 38; see also 73).

Schneider goes on to contrast his approach to cultural analysis with Geertz's. He sees the latter as bound—as Parsons himself has been—by Weberian assumptions: a domain of the *social system* (kinship or religion or economics or politics) is carved out, and the corresponding cultural realm is analyzed. A purely cultural analysis can fruitfully trace interconnections of symbols, premises, and principles of order wherever they lead; and a map of the cultural system *as a separate level* will, he argues,

look very different than an interpretation of the cultural correlates of social institutions. In the end, he calls for a pure cultural analysis "uncontaminated by the study of its social system"; and only after this logically prior task, for the tracing of interconnectedness between cultural, social, and psychological planes, so as to understand the social life of a people or the actions of individuals.

CULTURES AND SOCIOCULTURAL SYSTEMS

In seeking to clarify the issues that divide major theorists of culture, we begin with no expectation that an eclectic composite can be reached with which they would agree: any statement about culture on which Marvin Harris and David Schneider could agree would probably be vacuous. And being eclectic would lead back toward the broad and clumsy culture concepts of the past.

Nonetheless, a conceptual sorting out will be useful, not to reconcile the disagreements, but to identify their nature and source. Some of them are philosophical and some substantive; some could be resolved by empirical evidence, some could not. Each of the theoretical positions or approaches I have sketched has strengths and vulnerabilities. By underlining strengths and exposing vulnerabilities hidden beneath eloquent rhetoric, some ways of joining strength to strength and guarding exposed flanks, and some paths for future research, may usefully emerge.

A first contrast in sorting out these conceptualizations of culture parallels is drawn by Goodenough. I will call (with considerable precedent) the patterns-of-life-of-communities *sociocultural systems.* Sociocultural systems represent the social realizations or enactments of ideational designs-for-living in particular environments. A settlement pattern is an element of a sociocultural system, not an element of a cultural system in this sense. (The same conceptual principles might yield densely clustered villages or scattered homesteads, depending on water sources, terrain, arable land, demography, and the peaceful or headhunting predilections of the neighboring tribe.) A mode of subsistence technology similarly is part of a sociocultural system, but not strictly speaking part of a cultural system (people with the same knowledge and set of strategies for subsisting might be primarily horticulturalists in one setting and primarily fishermen in another, might make adzes of flint in one setting or shells in another, might plant taro on one side of a mountain range or yams on the other side).

What cultural adaptationists are talking about are, in this sense, sociocultural systems-in-environments. It is these systems that are adaptive or maladaptive, that are subject in some way to natural selection. Ideational designs for living, patterns of shared meanings and systems of knowledge and belief, are crucially important subsystems of ways-of-life-in-environments. The latter are complex systems in the cybernetic sense, in which complex circuits connect ecological, demographic, ideational, and other subsystems.[9] How these circuits are interconnected, how infor-

[9] That these subsystems, or elements of them, may be from different ontological realms is, in the perspective of cybernetics, irrelevant (66).

mation ramifies through them, and how homeostatic processes and directional change operate are (or can be) empirical questions for investigation, not articles of faith and ideological polemic.

[Note that this conceptualization of culture as an ideational system does not then correspond to the distinction drawn by Harris and some other cultural adaptationists between the economic domain (subsistence, technology, social organization of productive units) and the ideational realm (religion, ideology, law, art, etc). Knowledge and strategy about environments and ways of extracting subsistence from them, about making tools, about forming work groups, are as much a part of the ideational realm I am calling "culture" as patterns of cosmological belief or religious ritual.][10]

That throws Goodenough, Lévi-Strauss, Geertz, and Schneider in one camp (from which the clashing of symbols can be heard in the distance). It does so in a way that most new archeologists and ecological/evolutionary cultural anthropologists can probably accept as a possible—if not necessarily productive—conceptual strategy. At least they would mainly agree that what they are interested in are sociocultural systems[11] and how they develop and change. One can then investigate how ideational systems operate in this process of adaptation and change, both in terms of internal structure (how are changes in ideas about subsistence strategy related to changes in ideas about kinship or changes in ideas about religious ritual?) and in relation to other subsystems (how are ideas about choosing postmarital residence related to increased population or increased agricultural production?).

CULTURES AS IDEATIONAL SYSTEMS: PARADOXES AND PROBLEMS

The theorists of cultures as ideational systems, whom we have thrown into one noisy camp, remain to be sorted out. These modern theorists share an important premise that partly distinguishes them from their intellectual predecessors. As Singer (76) has noted, the two parallel traditions of American cultural anthropology and British social anthropology each entailed a kind of intellectual imperialism: for the former, social patterns were one realm of the culture; for the latter, especially Radcliffe Brown, cultural patterns are crystallized in social structure "as institutionalized and standarized modes of behavior and thought whose normal forms are socially recognized in the explicit or implicit rules to which the members of a society tend to conform" (76, p. 532). The dangers of swallowing the social into the cultural or the cultural into the social have been vividly portrayed by Geertz:

> Either culture is regarded as wholly derivative from the forms of social organization . . . or the forms of social organization are regarded as behavioral em-

[10] Note, however, that the distinction I have drawn is characteristically observed in Marxist analysis.

[11] Many, such as the Binfords, have used that designation as more or less interchangeable with "cultural systems."

> bodiments of cultural patterns. In either case . . . the dynamic elements in social change which arise from the failure of cultural patterns to be perfectly congruent with the forms of social organization are largely incapable of formulation (22, p. 992).

He, Goodenough, Lévi-Strauss, Schneider, and other major modern theorists share the premise that cultural and social realms are distinct though interrelated: neither is a mere reflection of the other—each must be considered in its own right. Such a conceptual untangling is basic to the refinements of theory and narrowings of the "culture" concept of the last 20 years.

The heart of the conceptual disagreements between these scholars is the problem of what to do about a basic paradox of human social life: When individuals engaging in social relations—even if there are only two of them—share common meanings, common understandings of one another's acts, then these shared meanings are greater than the sum of their "parts," their realizations in individual minds. Social meanings transcend, by some mysterious alchemy of minds meeting, the individuation of private experience. Social thinkers have struggled with this paradox for decades, even for centuries; yet *consciences collectives* still confound analytical dissection.

Goodenough's solution is to describe "culture" as an idealized systematization of an individual cognitive world, one that could enable an outsider to produce culturally appropriate responses in the range of social situations a native actor would encounter—to decide in culturally "grammatical" ways "what is, . . . what can be, . . . how one feels about it, . . . what to do about it, . . . and how to go about doing it." Thus what is shared is reduced to an idealized individual actor's point of view (one who, like Chomsky's hypothetical speaker-hearer, knows his culture perfectly). Goodenough's cognitive model would thus be a composite of the cultural knowledge of individuals in different social niches. Yet he, like the linguists, leaves room to deal with subcultural variations and individual differences (33, 34, 37). Goodenough is by no means as simplistic a cognitive reductionist as Geertz portrays him to be:

> People learn as individuals. Therefore, if culture is learned, its ultimate locus must be in individuals rather than in groups. . . . Cultural theory must [then] explain in what sense we can speak of culture as being shared or as the property of groups . . . and what the processes are by which such sharing arises. . . . We must . . . try to explain how this analytically useful construct relates to . . . the social and psychological processes that characterize men in groups (37, p. 20).

Goodenough carefully distinguishes seven related ideational senses of "culture" that systematically relate the cognitive worlds of individuals to the collective ideas and behavior of populations (37, pp. 41–42).

Lévi-Strauss sees cultures as transcending individual actors, even as transcending in a sense ethnic boundaries; yet collective representations reflect and reveal the structures and processes of the individual minds of which they are cumulative creations.

Geertz takes the alchemy of shared meanings as basic, but—following

Wittgenstein, Husserl, and Ryle—not as mysterious. Public traffic in symbols is very much of this world, not (he would argue) of a Platonic reified imaginary one. Geertz presumably would agree that cultures are "located in time and space by the temporal and spatial distribution of the individuals bearing them" (6, p. 86): but cultures are, as it were, *between* the minds of these individuals, not *in* them. Schneider wants to go a step further, it seems, toward a "methodological essentialist" position (63, pp. 28–29) that a culture in some sense exists "in its own right independently of its imperfect manifestations in the thoughts and actions of its bearers" (6, p. 86). In distinguishing the normative system from the system of cultural symbols and meanings, Schneider explicitly abstracts above and away from the perspective of an individual actor. This level of disembodied symbols, freed from their moorings in the world of social action and situational context, exists in the cognitive world of the cultural theorist; but only ego-centered *perspectives on it* exist in the cognitive worlds of his subjects.

This raises another facet of the basic paradox of the transcendence of shared symbols. Each actor perceives the way of life of his people as in some sense *external.* We do have a perspective on what we construe to be the game our fellows are playing (we also unconsciously assume—through cultural learning—many elements of the world in which we see it being played and view it with emotions subtly shaped by cultural experience). Perceiving "the system," one has some free rein to try to beat it, join it, change it, etc. (53).

Moreover, the immediate life space each of us mainly moves in is a world not of roles and institutions and abstract rules, but of individual people and places that are intimately known. We live our lives mainly in familiar phenomenological space whose particularities guide our response. We call cultural roles and rules—based on the general and abstract, not the concrete and individual—into play mainly on the periphery of our immediately familiar space, when dealing with strangers or bureaucracies, or engaging in passing encounters with salesclerk or policeman (encounters much less frequent in the small-scale societies of most of the human past). In this sense, the knowledge that enables individuals to act in "culturally appropriate" ways is only a part of what they know that enables them to live in groups.

There are, moreover, fuzzy but important differences between a collective ideational system and the psychodynamics of the individual—differences theorists of "culture and personality" have sought for years to untangle.

All this means that any effort to reduce cultural systems to the cognitive system of an idealized individual actor is fraught with danger.

> In such a way, extreme subjectivism is married to extreme formalism, with the expected result, an explosion of debate as to whether particular analyses . . . reflect what the natives "really" think or are merely clever simulations . . . The cognitive fallacy—that culture consists of "mental phenomena which can . . . be analyzed by formal methods similar to those of mathematics and logic" (80) is as destructive of an effective use of the concept as the behaviorist and idealist fallacies to which it is a misdrawn correction . . . Culture . . . is

no more . . . a psychological phenomenon, a characteristic of someone's mind, . . . than . . . Tantrism, genetics [or] the progressive form of the verb (Geertz 30, pp. 11–13).[12]

But the other horn of the conceptual dilemma—to cut "culture" free of the individual minds through which it is realized—poses dangers as well.

First, the structure of cultural systems is created, shaped, and constrained by individual minds and brains. What forms cultures take depend on what individual humans can think, imagine, and learn, as well as on what collective behaviors shape and sustain viable patterns of life in ecosystems. Cultures must be thinkable and learnable as well as livable.

Without informing our models of cultures with deepening knowledge of the structures and processes of mind, our cultural analyses may turn out to be mere literary exercises. Schneider complains that Geertz—in shaping his cultural analyses around religious institutions (or agricultural or economic ones)—distorts the patterning of cultures as ideational systems. But does Schneider, having moved to an ethereal level of symbols and meanings transcending individual minds, have a spurious freedom to draw his own designs when he thinks he is tracing other peoples'? Schneider has trained a number of students to expect great diversity in the cultural realm of kinship symbols; and not surprisingly, they have found it. Exploring a transcendent level of cultural symbols wherever they seem to lead, we may similarly discover radical diversity in other realms. But many of us are convinced that the diversity is to substantial degree a spurious artifact of the pursuit of cultural symbols unconstrained by the way humans think and learn and communicate, and the social settings in which they act. Time will resolve the question of whether kinship systems are in fact radically diverse symbolic realms, or permutations of the same basic system—so that an Australian Aborigine could drop in on New Guinea tribesmen or desert Bedouin and understand immediately, even through linguistic filters, what kinship discourse was about.

This raises, though obliquely, the question of universals. Are there in nonlinguistic culture universal patterns paralleling the ones emerging in language? In linguistics, the emergence of a universal grammar underlying surface syntactic diversity (and obviating any strong form of Whorfian relativisim) has been a major theme of the last decade. Especially in recent generative semantics, the deepest structures of sentences are seen as propositions in a universal "natural logic" that encodes meanings—a logic very similar to that formalized by Boole in his much-maligned *Laws of Thought* (1854) where he sought "to investigate the fundamental laws of those operations of the mind by which reasoning is performed . . . and . . . to collect . . . some probable intimations concerning the

[12] As noted, Geertz errs in attributing a naively reductionist cognitive view to Goodenough himself; for though Goodenough "places culture in the minds and hearts of men," he is in fact highly sophisticated in his discussion of culture as a composite of what is shared and public; and his most recent synthesis develops a view fairly close to the one I will attempt (37).

nature and constitution of the human mind." Lévi-Strauss has of course sought to find universal processes of thought in the stuff of cultures: he could well have used the same words as Boole to describe his own enterprise, though the linguistic models he borrows are mainly the relatively static formalisms of structuralist phonology.

What is important is that this seems an auspicious time to seek cultural universals, though not to assume them, for language may turn out to be a quite specialized subsystem of logic and transduction (48, 49). And what universals there turn out to be will—most interestingly, at least—be universals of process, of logic, of structure, of organizational principles, rather than of substance (48, 69). The sterility of seeking common denominators in the substantive stuff of culture has been eloquently argued by Geertz (25).

To what degree universals of language would reflect innately specified rules, logics, or structures is open to serious debate. Chomsky has argued for detailed innate specification; Piaget and others have countered that general cognitive principles and strategies may underlie both linguistic competence and acquisition of other cognitive abilities; and Piaget contends that highly complex hierarchical cognitive systems are built on minimally programmed foundations through the progressive unfolding of more and more complex "theories" about the world, each built on and reorganized from the previous one (62). If there turn out to be important universals of cultural structure (in this formal, not substantive, sense), it is not yet clear how much genetic programming and how much progressive cognitive refinement would underlie them.

Such frontier questions underline the urgency of not divorcing a conception of culture from our burgeoning knowledge of the mind. Geertz, concerned to bring the enlightenment of phenomenology, linguistic philosophy, and hermeneutics to anthropology, would do well to remember that it has been revolutions in science (evolution, relativity, quantum theory, cybernetics, molecular biology, linguistics) that have progressively transformed modern philosophy, not the reverse. A revolutionary advance in our understanding of the organization of intelligence—in a broad cybernetic sense that includes coding at a genetic, cellular, organismic, and ecosystemic level as well as in mind and brain—is now in its early stages.[13] In the international quest—not interdisciplinary but *superdisciplinary*—to unite a formal theory of intelligence and communication with an emerging theoretical biology and the empirical sciences of cognition (4, 60, 64), the human brain and its opposite face, the mind, represent the ultimate challenge, the most complex known natural systems:

> The human brain integrates the facts that it acquires through experience and other forms of learning into a model of the world. New facts are interpreted in the light of the model . . . Understanding . . . such world models, their

[13] Intelligence in this sense refers not simply to brains, real or artificial, but to formal representations of systems that display "biological" or "mental" properties of self-organization, goal-direction, and information processing characteristic of living systems. "Formal biology in this sense . . . would . . . be . . . a theory of *all* organisms, both natural and artificial" (51, p. 49).

neural organization, their dependence upon environment and culture, are fundamental and difficult questions that cut across many scientific disciplines (14, p. 437).

More than a decade ago, Geertz noted early advances on these fronts and their potential importance (23); and in 1965, he wrote that "culture is best seen not as complexes of concrete behavior patterns—customs, usages, traditions, habit clusters— . . . but as a set of control mechanisms—plans, recipes, rules, instructions (what computer engineers call 'programs')—for the governing of behavior" (25, p. 57). But he has not, I think, fully explored the implications of these insights. We will be poorly served if we weather the next revolutionary storms in philosophy poring over Husserl, Ryle, and Wittgenstein.[14]

Finally, treating the realm of cultural symbols as shared and public, as transcending the minds of individuals, raises the danger not only of the cultural interpreter creating a spuriously integrated and internally consistent symbolic design (recall Geertz's octopus analogy), but also of his hiding diversity and obscuring change. It seems likely that a range of diversity in individual versions of the "common" culture is not simply a social imperfection, but an adaptive necessity: a crucial resource that can be drawn on and selected from in cultural change. The most abstract cultural premises about what things there are and how they are related to one another and to human life may be relatively uniform and slow to change. But specific plans and patterns for human action and understanding are diverse (37, 78, 84), and unlike Beethoven quartets, they change.

But we are still left between the horns of a conceptual dilemma: on the one hand, of cognitive reductionism that misses the magic of shared symbols and the only partial overlap between the psychological world of the individual and the code of cultural meanings and conventions; and on the other, of a spuriously autonomous and spuriously uniform world of cultural symbols freed from the constraints of the mind and brain by which cultures are created and learned and through which they are realized.

TOWARD A CONCEPTUAL UNTANGLING

Perhaps the conceptual distinction between "competence" and "performance" that linguists are struggling to maintain can provide an avenue of escape from this dilemma. Linguistic competence is a model of the knowledge of his language a native speaker *draws on* in speaking and hearing (the processes of linguistic performance). In the Chomskyan linguistics of the mid-1960s, primary concern was with the competence of an idealized speaker-hearer who knows his language perfectly. But increased sophistication in the transformational camp, and pressures from

[14] As Bateson points out (10), there are serious problems of logical typing involved in the conceptualization of culture that have yet to be sorted out. Cybernetic modeling, drawing on increasing understanding of the formal structure of heterarchical systems, may be crucially important in this regard.

Labov and his fellow "variationists," increasingly lead theorists to deal with diversity. How and at what level the linguistic competence of individuals varies has become a hotly debated issue that will be carefully studied in the next few years. Whether we analytically create a uniform idealized competence or plot differences in the competence of subgroups (dialects) or individuals (idiolects) becomes a question of heuristic strategy geared to the problem of the moment. Competence remains distinguishable from performance.[15]

It seems potentially possible to distinguish analytically a *cultural competence* that does not incorporate the whole psychological world of each individual, and that allows us to avoid both horns of the conceptual dilemma.

Culture, conceived as a system of competence shared in its broad design and deeper principles, and varying between individuals in its specificities, is then not all of what an individual knows and thinks and feels about his world. It is his *theory of what his fellows know, believe, and mean,* his theory of the code being followed, the game being played, in the society into which he was born (see also 37). It is this theory to which a native actor *refers* in interpreting the unfamiliar or the ambiguous, in interacting with strangers (or supernaturals), and in other settings peripheral to the familiarity of mundane everyday life space; and with which he creates the stage on which the games of life are played. We can account for the individual actor's perception of his culture as external (and as potentially constraining and frustrating); and we can account for the way individuals then can consciously use, manipulate, violate, and try to change what they conceive to be the rules of the game. But note that the actor's "theory" of his culture, like his theory of his language, may be in large measure unconscious. Actors follow rules of which they are not consciously aware, and assume a world to be "out there" that they have in fact created with culturally shaped and shaded patterns of mind.

We can recognize that not every individual shares precisely the same theory of the cultural code, that not every individual knows about all sectors of the culture. Thus a cultural description is always an abstracted composite. Depending on the heuristic purposes at hand, we, like the linguists, can plot the distribution of variant versions of competence among subgroups, roles, and individuals. And, like the linguists, we can study the processes of change in conceptual codes as well as in patterns of social behavior (37).

Such a conception of culture as an idealized body of competence differentially distributed in a population, yet partially realized in the minds of individuals, allows us to bring to bear a growing body of knowledge about the structure of mind and brain and the formal organization of intelligence. Even though no one native actor knows all of the culture, and each has a variant version of the code, culture in this view is ordered

[15] The main difficulties have come in maintaining the boundary between linguistic competence and cultural knowledge in the face of the need to study presuppositions. The permeability of that boundary need not concern the anthropologist, who begins on the messy side linguists want to avoid if they can (48, 52).

not simply as a collection of symbols fitted together by the analyst but as a *system of knowledge,* shaped and constrained by the way the human brain acquires, organizes, and processes information and creates "internal models of reality" (16, 38, 39). Such a conception of culture frees us potentially from the dangers of both cognitive reductionism and ethereal idealism.

To this point I have suggested that we look to linguistics for conceptual guidance. However, the linguists—having distinguished competence from performance—have chosen mainly to study only the former. This has not only given modern linguistics an aura of ivory tower scholasticism in a world where language has been an instrument of oppression and a force of division. It has unnecessarily separated advances in linguistics from advances in psychology, anthropology, and biology. And as Labov and others have pointed out, it has progressively narrowed the data base of linguistic inquiry so that a large edifice teeters precariously on a thin edge of intuition.[16]

I am convinced that if anthropologists conceptualize culture as epistemologically and logically parallel to linguistic competence, they should do so only within a wider concern with sociocultural "performance." An ideational conception of culture "will serve us badly if we take the abstract system we have created out of the flux of the phenomenal world and examine 'it' to see how 'it' is put together. But 'culture' could serve us well if we use it to help untangle the vastly complex skeins of interconnectedness in that world" (48, p. 326).

I am thus agreeing with Schneider that cultures as ideational systems should be explored and mapped in their own terms, not in terms of the domains of social life; but I am disagreeing with his conclusion that the study of culture can profitably be pursued "uncontaminated" by the study of the social and ecological settings in which humans act.

Let me make my reasons explicit.

1. The questions that mainly concern anthropologists are only partly questions about cultures as ideational systems. We want to understand how human groups organize and sustain their social life; how biology and experience interact as individuals become functioning members of a society, and how the nature of that experience shapes personalities; how different—and how similar—are human modes of thought and perception in different times and places; how ways of life change, and what shapes the form they take in particular settings.

We cannot understand other people's lives simply by mapping their culture—though (contra Harris 40) we cannot understand or even adequately record events in their world without understanding their "internal models of reality" (see 15, 37). I have elsewhere illustrated this with Trobriand examples (47, p. 404; 50, p. 441). A competence model of Trobriand culture would tell us what classes of things, people, and events there are and what kind of a world they are situated in, and it would give rules for how to garden, trace descent, exchange, and reside. But

[16] Or more precisely, on native speakers' competence in communicating their intuitions about sentences proferred by a linguist.

it would tell us nothing about residence patterns, descent groups, agricultural production, or the flow of exchange—or even how many Trobrianders there are and where they live.

2. The magic of shared symbols, of minds meeting, is not a magic that occurs on some ethereal cultural plane; as Geertz, phenomenologists, and ethnomethodologists are vividly aware, it is a magic enacted in social settings. It is embedded in public encounters. "The mind is not even a metaphorical 'place' . . . The chessboard, the platform, the scholar's desk, the judge's bench, the lorry-driver's seat, the studio, and the football field are among its places" (70, quoted in 23). Meanings are shared by people whose conceptions of their culture are not identical; and that is more than a matter of common denominators or even of "equivalences" (84). But it is a magic achieved not in a hypothetical vacuum, a symbolic realm, but in the collective application of the general to the particular, the private to the social.

3. To understand change and diversity, we must see cultures as elements in complex cybernetic systems of humans-in-environments. An ideational model of culture, in isolation, prevents our understanding change and adaptation. As part of a more complex conceptual scheme, however, such a model of culture enriches our understanding of change and helps us to correct overly simplistic ecological/adaptationist models.

Cultures must generate viable patterns-of-life in ecosystems (or more precisely, they must not generate nonviable ones). But that does not mean that natural selection prunes and shapes ideational systems in any simple and direct way. Patterns of social life in a community are not a simple enactment of shared cultural programs. As Homans has observed, "the central problem of the social sciences remains that posed, in his own language and in his own age, by Hobbes: How does the behavior of individuals create the characteristics of groups?" (46, p. 106). The behavior of individuals is guided, channeled, and constrained by cultural principles and rules about the game of life and how it is to be played. But it is individuals, making choices, pursuing strategies, maximizing values, forming coalitions, that generate the patterns of social life[17] (5, 7–9, 46). The rules of the game are themselves generated and changed by the patterns of play they guide, in a continuing dialectic.

It is how humans live, not how they conceptualize the game of life, that is directly shaped by selective pressures. Moreover, the superbrain that enables humans to solve survival problems in a wide range of environments imposes costs of its own: ritual, myth, cosmology, and magic may be adaptations to the pressures of the human psyche—to anxieties, frustrations, fears, and questionings—as much as they are adaptations to the pressures of the external environment.

4. To study cultures as ideational systems without mapping the complex cybernetic circuits[18] that link them to social systems, to ecosystems, and to the psychology and biology of individuals would turn cultural

[17] Cf Freilich's distinction between "proper" and "smart" in his rather different conceptualization of the cultural and social realms (21).

[18] In the sense explored by Rappaport (66–68).

analysis into an arcane pursuit isolated from surrounding disciplines at a stage when a fantastic burst of scientific knowledge—with human survival as the stakes—is being launched: a burst that should relegate to the realm of Ptolemaic astronomy (or at least pre-Watson-Crick genetics) previous theories in ecology, the neurosciences, psychology, and related fields.

5. In the course of this advance, an irony may loom increasingly large: cultures as systems of knowledge may turn out to be only partly describable in the formal languages we command. Despite impressive progress in cybernetic modeling of the way the central nervous system processes and organizes information, there is a vast gulf between the models and what the brain achieves efficiently and almost instantly. Some progress is being made to close this gap, and to unravel the mysteries of the living brain[19] (see, e.g., 4, 64).

But even as scientists begin to write ethnographies for robots and to explore mathematically and biologically the structure of "memory" (61, 64, 79)—of internal models of reality—many facets of mind resist formal representation. Interestingly, it is not the highly intellective logical functions of mind, but the evolutionarily old, unconscious, "automatic" functions that resist analysis.[20]

That suggests that there may be some fundamental obstacles, perhaps more evolutionary than Gödelian, to our laying bare in any formal way what humans "know" that enables them to do what they do. George Miller's warnings vividly suggest the dilemma:

> Given that we can know rules that have not yet been formulated [as in our implicit knowledge of grammatical rules], could we know rules that govern the operations of the human mind that the human mind, given its present level of intelligence and symbolic machinery, cannot make explicit? (58, p. 192).[21]

The point is not to digress about artificial intelligence research, but to warn that despite a vast concentration of brainpower, the possibility of analyzing a cultural system in any complete sense and of discovering and describing its structure remains far on the horizon—and may forever remain so. To abstract out a level of "cultural symbols" in the way Schneider proposes seems to me to offer a spurious sense of escape from

[19] Cybernetic modeling helps to clarify the relationship of "mind language" to "brain language"; but as MacKay (55, p. 465) argues, even if all operations of mind could ultimately be linked to processes of brain, there would still be an important need for "mind language." Neurophysiological reductionism is as defeating as any other form of reductionism.

[20] "Good progress has been made in the art of programming. For instance, it took an automaton only a few minutes to prove over 200 theorems from Whitehead & Russell's *Principia Mathematica,* some of these proofs being even more elegant than the known ones. But the robot's ability has peculiar limits. For example, no automaton has so far been built which in the matter of reading handwritten addresses can match even a mediocre post office sorter . . . Some functions . . . having a primitive and far from intellectual nature, are much more difficult to automate than certain other functions which we regard as typically intellectual. . . . *It is for those functions which take place unconsciously that no satisfactory automata have been built*" (74, p. 46; cf 17, 85).

[21] Von Foerster's cryptic remark expresses the same insight: "The Laws of Nature are written by man. The laws of biology must write themselves" (82, p. 5).

this dilemma. That the anthropologist's mind can invent such a "level" attests to the remarkable powers that make humans human; but it does little to clarify how they perceive, think, and act.

It is partly Geertz's realization that the cultural grammars of the "new ethnographers" are so impossible to achieve in the face of the vast intricacy of what humans know about their world—the subtle shadings of understanding and mood and meaning that defy representation in formal algorithms—that leads him to aspire at most to thick description, to interpretation rather than "decoding" or explanation. I disagree with him if that means abandoning to cyberneticians the task of progressively filling in those segments and sectors that yield to understanding: they will do so less well without our collaboration than they would if we shared with them our insights into the varying patterns and richness of cultural experience. But I agree with him that as those enterprises advance we must remain rooted in the immediacies of interpreting real humans in real settings.

6. A final urgent argument for embedding an ideational conception of culture in the real social and ecological world is that "culture," like other heuristic concepts of social science,[22] should be potentially self-extinguishing. Like the linguists' notion of competence, it may in the longer run turn out to be a scaffolding that needs to be dismantled when more solid and enduring structures can be built.

It remains an open question to what degree human action actually is guided by a general conceptual code, a theory of the world and the game of social life that can be disentangled from the particularities and immediacies of each individual's unique experience and life space. John Haviland's recent study of gossip in Zinacantan from a cognitive perspective poses important doubts:

> We ordinarily have thought of one's cultural competence as composed of codes. . . . The conceptual schemata have, we assume, an independent existence prior to any particular configuration of animals, any set of actual kin, any actual political operation. . . . But in gossip the . . . contingencies determine the general principles—for they are all there is. In gossip, the world becomes more than ideal schemes and codes. . . . Much of an actor's cultural competence rests on a vast knowledge of contingent fact, raw unconnected trivia. . . .
>
> Watching people operate on their cultural rules through gossip also shows us the folly of our belief that culture *provides* sets of ideal rules which apply to particular configurations of people, places, things, and events. The contingencies of life themselves restructure the rules, even change them in time . . . In gossip . . . one's whole understanding of the cultural code depends on the particular setting, on the configuration of past experience and knowledge, which is suddenly relevant to the application of rules and standards to the fact in question (43, pp. 279–80).[23]

Do human actors conceptualize "the system" in some systematic way and use this generalized model to guide action and understanding in

[22] See Bateson (10) on "dormitive principles" in social science.

[23] Quoted with permission from Dr. Haviland's PhD dissertation (43), which is being extensively revised for publication as a book.

concrete social situations? If not, a generalized composite model of cultural competence will in the long run serve us badly in understanding performance in the concrete settings of real life. We do not yet know.

Haviland reaches a conclusion similar to Geertz's: that at least for the present, we can best aspire to understanding and interpretation, not to prediction and explanation. (Gossip, at once text and native commentary on texts, offers particularly rich insights.) Moreover, it may be precisely in exploring the phenomenological world of the familiar and immediate, the everyday and mundane, that we stand to gain the most crucial knowledge of how humans perceive, understand, and act.

CONCLUSION

We need to work, I think, on many fronts. Interpreting cockfights in Bali and gossip in Zinacantan illuminates the human condition from one important perspective, even though—or perhaps *because*—what makes it possible for anthropologists and participants cannot be neatly codified. Studies of ritual and ecological adaptation in New Guinea illuminate another side, an interconnectedness we would, with less broad view of the systemic complexity of nature, have missed. At the same time, attempts to map cultures as ideational systems in the light of an emerging understanding of mind and brain should enable clearer insights into the organization of experience and the nature and depth of variation in the thought worlds of men.

Conceiving culture as an ideational subsystem within a vastly complex system, biological, social and symbolic, and grounding our abstract models in the concrete particularities of human social life, should make possible a continuing dialectic that yields deepening understanding. Whether in this quest the concept of culture is progressively refined, radically reinterpreted, or progressively extinguished will in the long run scarcely matter if along the way it has led us to ask strategic questions and to see connections that would otherwise have been hidden.

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3

Culture and Cognition

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The long-standing dispute among anthropologists over the relative merits of "emic" and "etic" in ethnographic description is essentially an argument over a behavioristic versus a cognitive definition of culture. A similar, and independently evolved, clash of traditions has existed in experimental psychology for some time, the learning theorists (such as Pavlov, Hull, Skinner, and Osgood) explaining behavior by reference to one or another model of directly observable stimulus-response-reinforcement sequences, and the cognitive theorists (the Gestalt school, the psychodynamic tradition, and such recent experimental investigators as Jerome Bruner and Leon Festinger) explaining it in terms of schemata stored in the brain as a result of learning and inferable from performance and verbal report. Since much of ethnography is, from the standpoint of this book, a description of cognitive systems, it is desirable to review certain aspects of the issue in psychology for the light it may shed on anthropological approaches to cognition.[1]

SOME ASPECTS OF THE PROBLEM OF COGNITION IN EXPERIMENTAL PSYCHOLOGY

The so-called S–R and information-processing approaches to cognition are both confronted with the problem of accounting for certain phenomena of learned behavior that have been demonstrated in laboratory experiments. Conspicuous among these phenomena are "stimulus equivalence" and "response equivalence."

"Stimulus equivalence" is the tendency of a subject to respond in the same way to different conditioned stimuli (not merely objectively different by finely honed technical measurement but different within the capacity of the subject to discriminate). For instance, the actual stimulation of receptors (sense organs) by a given object or event may vary widely depending on such factors as intensity, distance, lighting, and position in space; merely "similar," and not identical, stimuli are adequate to elicit a given response; the condition of the organism with respect to hunger, fatigue, stress, and so forth, may vary widely without obstructing performance. In other words, the organism "generalizes": it classifies a range of objectively different stimuli as equivalent. In human beings, this is impressively demonstrable in semantic conditioning. In these experiments, a human subject learns to salivate, or respond in some other

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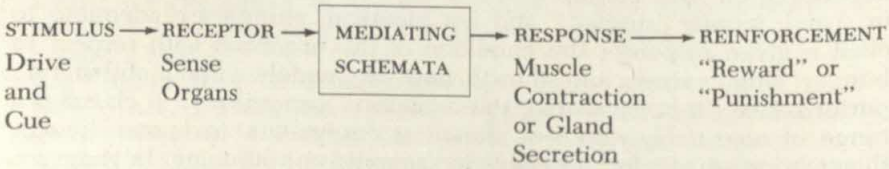
† Source: *Culture and Personality*, 2d ed. (New York: Random House, 1970), pp. 75–84.

[1] For psychological approaches see Bruner, Goodnow, and Austin, *A Study of Thinking*, and Harper, *et al.*, *The Cognitive Processes: Readings;* for a selection of anthropological writings see Tyler, *Cognitive Anthropology*, and Garvin, *Cognition: A Multiple Approach*.

reflex modality such as vasoconstriction or blood coagulation time, in response to a particular word or phrase. Acoustically identical but semantically different words, visually presented, do not evoke the response. Acoustically different but semantically identical (or at least closely related in meaning) words do evoke the response. Thus to a subject conditioned to drool at the words "style," "urn," "freeze," and "surf," the words "stile," "earn," "frieze," and "serf" are not stimulating; but his mouth waters at "fashion," "vase," "cold," and "waves."

Correspondingly, "response equivalence" may also be regarded as a kind of classification. For example, Bruner and his associates over a period of time trained rats to run a certain maze whose pattern was *LRLRLR* . . . Those rats who had learned this pattern of responses thoroughly took far less time to learn a maze whose pattern was reversed: *RLRLRL* . . . The experimenters concluded that the rats had learned not just the original *LRLR* pattern, but a more general principle (whether it was acquired during the learning of the initial sequence or after the first few trials on the new sequence is not clear). On the human level, a similar point has been urged by Noam Chomsky[2] who argues convincingly that because a person can produce an infinite number of grammatical sentences in his language, he has not learned particular strings of words, or parts of speech, or even a stochastic matrix for generating such strings, but rather a basic grammatical structure from which, by applying transformation rules, he can generate any number of sentences. Similarly, Miller, Galanter, and Pribram[3] and others point out that often a "response" is not a particular and finite sequence of muscular contractions but an open-ended effort to achieve a given effect on the environment by one means or another.

Such considerations as these lead both learning theorists and cognitive theorists to look at the old paradigm anew. Evidently it is the mediating schemata that reinforcement reinforces, rather than a unique relationship between a single sequence of receptor images and a single sequence of muscular contractions or glandular secretions. Hence one must ask what previously had not been a serious question: What are the mediating schemata—structures or processes—in the brain that must be assumed to account for such phenomena as stimulus equivalence and response equivalence?



The learning theory answer in general is that the brain contains complex S–R circuits, including internal reinforcers, which are somehow estab-

[2] *Language and Mind.*
[3] *Plans and the Structure of Behavior.*

lished in learning and are operative in such "cognitive" processes as perceptual classification and planning. To speculate about such circuits is awkward, however, for learning theory has prided itself on its objectivity and reliance on the measurement of hearable, seeable, countable physical objects and physical events; yet now it must speculate on the nature of physical objects and events that are not hearable, seeable, or countable in order to preserve the claim to universality of explanation. The cognitive theory answer in general is to concentrate attention on "what is learned" and to describe information-processing structures (also denoted as schemata, plans, strategies, classifications, images, maps, codes, and so forth) on the basis of inference from performance (and, in the case of human beings, to some extent from introspection and verbal report, in the language in which such structures are customarily communicated by one person to another). It even becomes permissible to question whether reinforcement in any but the most metaphysical sense is necessary for all learning or for the preservation of schemata from extinction (*vide* curiosity learning, latent learning, and imprinting).

But the last question requires a corollary. If, as both human intuition and laboratory experiment suggest, reinforcement is *not* necessary for all learning, knowing, and thinking, then even where reinforcement *is* at work, there may be another process that contributes to the formation and maintenance of schemata. Thus, the corollary question is: What processes are there in addition to S–R learning that might contribute to the formation, maintenance, and application of schemata? Two sorts of answers suggest themselves, which are not mutually exclusive, and both of them of traditional interest to anthropologists: first, there may be certain cognitive capacities for forming kinds of schemata that are neurologically "wired into" all human beings, are genetically determined, and are presumably a product of evolution; second, there is a phenomenon that might be called "autistic thought" (without implying necessary psychopathology) that is responsible for reorganizing what has been learned, constantly recombining and differentiating elements in novel arrangements, going beyond information given, solving contradictions, and sometimes innovating new schemata. The first kind of answer is explicitly asserted by Chomsky for the capacity to form a grammar and by Claude Lévi-Strauss for certain principles of classification and analogical and dialectical reasoning. The second kind of process (for which the capacity, to be sure, is assumed to be innate and biologically based) is suggested by such familiar phenomena as dreaming, meditating, and thinking; personality development by the use of defense mechanisms and psychotherapy; and "creative" work of various kinds. ("Autistic thought" in the pathological sense then would be merely a condition of preoccupation with autistic thought in a person whose automatic synthesizer is not producing adequately.)

Indeed, it is difficult to dispense with the notion of an automatic synthesizer, which mechanically manufactures possible new schemata, as a contributor to the formation of schemata even in an S–R model of cognitive process. For the automatic synthesis must play the role, in any mental process in which new environmental information is *not* coming in, that

the experimenter plays in the typical learning procedure where cues and reinforcements are environmental. The *experimenter* (whose role is usually not adequately represented in psychological analyses of the learning process) himself invents the alternate schemata that are imposed by design and schedule on the subject—rat or man—for reinforcement to select. Inside the experimenter's head, however, there is no homunculus to perform the task he performs for the rat. Without an automatic synthesizer to produce alternates as grist for the reinforcement mill, it is difficult to see why reinforcements are needed at all. The problem is not unlike a problem in the theory of natural selection: just as Darwin had to assume a supply of "variations" among which selection could select, so the learning theorist must assume variable behavior in the subject—subject must be able to do at least two things in any situation—in order for reinforcement to select (if there is no variation, reinforcement would be gratuitous; thus, a simple reflex is not learned and need not be reinforced). But where does the supply of variations come from? For the evolutionist, the answer has been mutation (and, for some, acquired characteristics); for cognitive theory, the answer has to be an automatic alternate-schemata synthesizer. And this answer is relevant to anthropology, for the cultural anthropologist must also ask: How are the innovations produced that lead to culture change?

THE GENERAL CHARACTERISTICS OF SCHEMATA

With relatively little intercommunication on the subject, and for the most part (but with some interesting exceptions that we shall discuss later) working on different bodies of data, anthropologists, linguists, and psychologists have come to rather similar conclusions about the general characteristics of the schemata that they discover. The dimensions on which these general characteristics are located are four in number: level of abstraction, degree of complexity, the nature of interschema linkages, and extent of verbalizability.

Any kind of "concept-formation," whether or not the concepts are represented in language or merely demonstrated in stimulus and response equivalences, by definition implies abstraction. The abstraction involves a restriction of attention to selected dimensions both of the environment and of the organism's own response potentialities, and the exclusion of others as irrelevant; it necessarily also involves the ignoring of variations within the minimum resolution range permitted by the physiology of the animal. In these senses, the schemata are both insatiable and inexhaustible: an unlimited number of different actual stimulus situations can be recognized as the same, and an unlimited number of actually different responses can be generated by the same schema. Furthermore, it seems to be the intuitive practice of both anthropologists and psychologists to "empty" the schemata they describe as much as possible of particular content and if possible to describe it as a "total" abstraction with minimal restrictions as to context. Thus, Bruner describes the rat schema as "single alternation" in the most general terms, although he does raise the question of whether it applies only to walled mazes or whether the rat would

apply it to, for instance, color alternation on a painted board. Linguists do their best to describe languages in terms free of particular utterances or of "meaning"; a list of phonemes does not specify a lexicon and a grammar does not contain any single sentence. Lévi-Strauss[4] intuitively proceeds to treat "a myth" as the set of all its variants and to describe "it" as a transformation rule describing the relationships among elementary themes; indeed, he argues that all myths are generated by one single schema represented by the mystic formula:

$$F_x(a) : F_y(b) \simeq F_x(b) : F_{a-1}(y)$$

Whether the formula adequately represents the schema of myth may be argued, but it represents very well the effort to "empty" schemata and to represent them in a content- and domain-free algebraic or geometrical formula. The same principle applies to the work of anthropologists on kinship systems: the terminological structures, and the rules of marriage, residence, and descent are as soon as possible reduced to abstract forms whose structures can be represented in a symbolic notation usually derived from logic, algebra, or geometry.

The use of such totally abstract notations, however, begs a question of importance: In what domain of experience is the schema valid? Let us return to Bruner's rats. What *did* they learn? Was it the abstract series *ababab* . . . , potentially discoverable as a progressive shortening of transfer learning time not only in the color alternation experiment but also in such conceivable other experiments as learning to press two bars alternately, then learning to lift first one paw and then the other, then learning to stand first in one corner and then the other, and so forth? Or is the domain restricted to turning left, then right, wherever the stimulus also has the form *LRLR* . . . ? Or is it restricted simply to pairs of *LR* mazes differing only in the direction of the initial turn?

Similar questions are of interest to the anthropologist, who tries to identify the same formal structure in diverse domains. Given an analysis of a kinship terminology and the description of its taxonomic structure in an algebraic notation, are the formal characteristics of the taxonomy (such as, for instance, a perfectly orthogonal matrix) found also in other taxonomic domains in the same culture such as plant classifications, color terminology, and so on? Are the same dimensions repeatedly used for concept formation in diverse domains? The continuing currency of interest in such descriptive concepts as values, national character, ethos and eidos, world view, the Sapir-Whorf type of speculation, and so forth, all depend upon an intuitive feeling that certain schemata are so "basic," so "deep," and so abstract that they give structure to many domains of experience and thus can be said to characterize the culture as a whole.

All this suggests that schemata have a kind of nesting structure, the deepest being the most abstract and least restricted as to domain, and the most superficial being the most concrete and most similar to environmental stimuli and responses in its imagery. For instance, in respect to taxonomy, the deepest level is given in the abstract structure of the type

[4] "The Structural Study of Myth."

of semantic space on which taxonomies are constructed; next, perhaps, one finds the dimensions as such identified for various domains; next, the variability characteristics of the dimensions are specified; next, the intersections and other logical relationships are detailed; and at the surface the appropriate, verbalizable lexicon is assigned to the taxonomic structure.

With respect to level of complexity, it appears that, for human beings, at any level of abstraction, similar conclusions are reached from both laboratory and ethnographic evidence. Speaking about discrimination responses, George Miller[5] has suggested the "magical number seven" as the comfortable limit for discriminations on a scale and for the number of binary dimensions that can be conveniently handled in concept formation. Working with cultural data, Wallace suggested a 2^6 rule: that folk taxonomies are typically built out of contrast sets requiring no more than six binary dimensions.[6] With respect to continuous variables, most human beings (I think) have difficulty in handling more than three dimensions simultaneously in mental arithmetic (although of course the use of writing and other objective symbols, models, and computational aids makes it possible to go far beyond this). There is no particular reason why, when working with discrete variables, the size of contrast sets should be in the neighborhood of 2^6 or 2^7 cells, other than the limitations of the neurological equipment involved. One is reminded in this connection that the information processing equipment in the individual cell (the "genetic code") also operates at the 2^6 level; and one may speculate that if this equipment is involved in cognitive processes, the human capacity for concept formation and that of simpler species is actually the same, being restricted to building blocks (contrast sets) of constant size (2^6), the human advantage lying simply in the capacity to link contrast sets in enormously larger structures.

We are now led directly to the question of relationships among schemata. As we have already suggested, one kind of relationship is certainly formed by extending a given schema over other domains. This principle is invoked repeatedly by Lévi-Strauss who sees it operative in analogical and metaphorical reasoning not only in "the savage mind" but also among his sophisticated and scientific contemporaries. He points out, for instance, that many "totemic" cultures use the taxonomic structure applied to the local flora and fauna as the conceptual schema for human groups: the names of animal species will be used to denote human sibs, and the relationships among the clans will be construed as being of the same kind, involving the same dimensions, as the relationships among the species: thus,

clan 1 : clan 2 ≈ species A : species B

But while this phenomenon does occur—and is, indeed, of great importance in theory development among scientists as well as among Australian

[5] "The Magical Number Seven, Plus or Minus Two: Some Limits on Our Capacity for Processing Information."

[6] "On Being Just Complicated Enough."

aborigines—it hardly exhausts the human capacity for formulating relationships among schemata. There is, first of all, in taxonomic schemata themselves, an impelling tendency to arrange such structures in the form of hierarchical graphs or trees, whose nodes consist of contrast sets and whose successively higher nodes consist of more and more abstract values representing the domains differentiated below them. Such structures are certainly visible in the belief systems embodied in culture. Relationships of contradiction and dissonance also exist, sometimes in happy insulation and sometimes in conflict, and much human energy would seem to be devoted to the process of relating such contradictory schemata, either by resolving the oppositions or by dissociating the schemata so that the contradictory schemata are not simultaneously invoked. Clinical psychologists (whether behavioristic or psychodynamic) concern themselves actively with such structures in the individual; anthropologists outside the Lévi-Strauss tradition have tended to assume that such contradictions do not occur in culture except between sociologically distinct subgroups, such as youths and adults in the socialization process, or progressive and conservative factions in acculturation situations, and that here they are either normally handled by ritually institutionalized defense mechanisms or by cultural reformulations.

Finally, I think it is generally agreed that the relatively "deep structures," the very general and very abstract schemata, are not really verbalizable by individual subjects without a great deal of coaching by an investigator who is inferring hypothetical schemata and feeding back his inferences. The psychologist does not expect his experimental subject, whether rat or man, to be able to describe the schema that he has learned in clear, exact, English prose. The linguist does not expect his informant to be able to expound in abstract terms the phonemic, morphophonemic, and syntactical system that actually guides his language behavior. The psychotherapist does not expect a new patient to be able to understand the symbolic equations and the interpersonal strategies by which he strives to avoid anxiety and attain libidinal satisfaction. The ethnographer does not expect to find an informant who can clearly and succinctly describe the semantic structure of his kinship terminology or precisely articulate the values of the community. All these schemata are "unconscious" in the sense that they cannot be stated in abstract form by the subject even though his perceptions and responses are precisely determined by them; they must be inferred from behavior, both verbal and nonverbal.

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* Compiled by R. W. Casson from the Bibliography of Wallace's book.

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II

Folk Classification: Relativity and Universality

ETHNOSEMANTICS

As was stated in the General Introduction, one of the major concerns of cognitive anthropologists is the study of systems of folk classification. This area of research, which in its early phase was labeled ethnoscience and now is generally termed ethnosemantics or ethnographic semantics, will be the focus of attention in this section. We shall consider the structure and organization of conceptual categories in folk classification systems, the analysis of the meaning encoded in these systems, and then aspects of relativity and universality in these systems.

In all societies, objects, acts, and events—that is, entities in the world—that are in fact different are grouped together in categories and regarded as generally equivalent. Every flower is in some respects unique, yet differences are ignored and flowers are classed as instances of particular cultural categories—for example, marigold and tulip. The number of conceptual categories in a cultural system is immense. The categories range from very specific and comprised of few members, to very general and composed of many members. They are not a chaotic jumble of unconnected concepts, but rather are interrelated in a network of complex linkages.

No analysis has yet been made of the full set of conceptual categories in a culture, just as no complete analysis has yet been achieved of all the conceptual units in a language. Like linguists, cognitive anthropologists have limited the scope of their investigations. Rather than attempt superficial accounts of whole cultures, they have concentrated on the study of conceptual categories called *semantic domains.* These domains are large cultural categories that contain a number of related smaller categories. Domains that have been investigated in particular cultures include plants, animals, colors, foods, kinsmen, statuses and roles, occupations, supernaturals, body parts, and event stages, among others, as is indicated by the contents of this book.

TAXONOMIC CLASSIFICATION SYSTEMS

One variety of folk classification system, probably the most commonly occurring in cultural systems and certainly the most commonly studied by anthropologists,

is the taxonomic classification system, or taxonomy. The ubiquity of this type of classification system is evident in the fact that the word *taxonomy* is used in two senses: in the more general sense, it refers to any type of folk classification system, and in the more specific sense, it refers to a particular type of classification system based on the "kind of," or "inclusion," relation. Before describing other types of classification systems, we will take a detailed look at taxonomic classification systems. The aim here will be not only to achieve an understanding of taxonomic systems, but also to illustrate the basic concepts involved in the study of all classification systems, and to provide a basis for comparing taxonomic with other less extensively studied types of classification systems. Chapter 4, the article by Brent Berlin on Aguaruna folk botany, is the relevant reading.

The structure of a taxonomy (or any other classification system) is determined by the relations among the categories that comprise the system. The notion of *semantic contrast* is basic here. Two categories are said to contrast when they differ in their member entities. They may contrast totally, in some or many respects, or in only one respect. In our culture, the categories associated with the lexical items *hamburger* and *truck* are totally distinct; *hamburger* and *chowder* contrast partially in that both refer to categories of food; and *hamburger* and *cheeseburger* designate categories of food that differ only with respect to the presence or absence of a slice of cheese (this now classic example is from Frake 1962).

Taxonomic classification systems are constructed on the basis of the type of semantic contrast that is termed *hierarchical inclusion* (also called *hyponymy* and *supraordination*). Categories that contrast in one or many, but not all, respects (and are therefore semantically distinct) may stand to each other in an inclusion or "kind of" relation—category X may strictly (that is, completely) include category Y, which is regarded as a kind of category X. For example, the category lexicalized *tree* in English includes the *oak* category, and an *oak* is a kind of a tree. Other categories included in the *tree* category are *maple, pine, elm, spruce, poplar, walnut,* and *fir.* The *tree* category is included in the superordinate category *plant,* as are the categories, *bush, grass,* (herbaceous) *plant,* and *vine,* and *oak* includes the subordinate categories *white oak, post oak, red oak, jack oak, scrub oak, live oak, pin oak, burr oak,* and *blue oak.* These categories comprise a hierarchical taxonomic structure, a fragment of our ethnobotanical classification system, which may be diagrammed as shown in Figure 1 (connecting lines represent inclusion relations and dots indicate omissions).

Categories in taxonomic structures, which are generally termed *taxa* (singular, *taxon*), exhibit other relations in addition to strict inclusion (cf. Kay 1971). Three of these need to be described. *Direct contrast* is the relation between taxa included in the same immediately superordinate taxon. The categories *oak, maple, pine, elm, spruce, poplar, walnut,* and *fir* are all immediately included in (or dominated by) the *tree* category and therefore are in direct contrast. Direct contrast is particularly significant because it provides the basis for defining the concept *contrast set,* which is simply the collection of taxa immediately included in a single superordinate taxon—for example, {*tree, bush, plant, grass, vine, . . .*} and {*white oak, post oak, red oak, jack oak, . . .*} (Braces enclose contrast sets and dots indicate omissions.)

Indirect inclusion is the relation between taxa that are not in direct contrast (and therefore are not members of a single contrast set) but rather are included

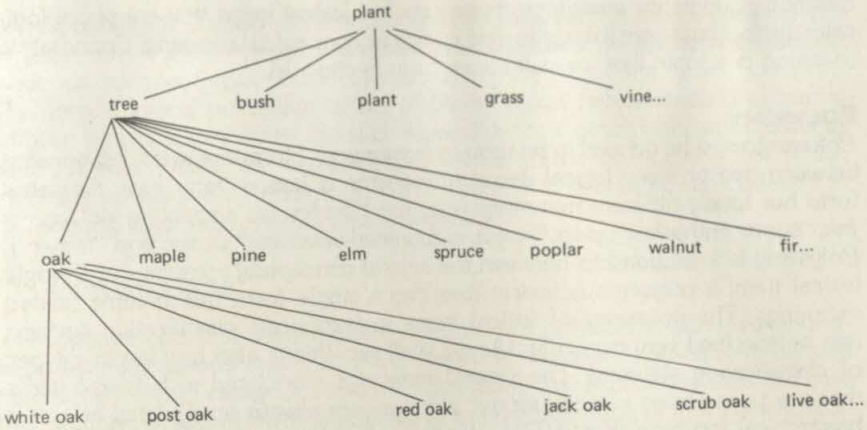

FIGURE 1. Ethnobotanical Taxonomy.

in taxa that are in direct contrast. *White oak* and *sugar maple* (a kind of *maple* not shown in Figure 1) are in indirect contrast and are members of distinct contrast sets. *Terminal contrast* is the contrast relation between any two taxa in the taxonomic hierarchy that domainate no subordinate taxa. Entities categorized in a taxonomic classification system are included in one and only one terminal taxon, so terminal taxa provide the finest partitioning of the semantic domain.

Thus far, we have been concerned with the relations among conceptual categories, or taxa, in taxonomic structures. Although the phonological representations associated with conceptual categories in lexical items may be used to facilitate discussion, conceptual meanings are distinct from the phonological forms that represent them, and semantic contrast is defined in terms of conceptual categories, not phonological manifestations. Taxa are always mutually exclusive, each one having a unique place in the taxonomic structure. The relation between semantic categories and their phonological representations must be determined empirically because meaning and form are not always associated in a one-to-one fashion. A single concept may be associated with more than one representation, and more than one concept may be associated with a single representation. The former relationship is known as *synonymy*—the "same meaning" is associated with different forms—and the latter is known as *polysemy*—"several meanings" are associated with one form.

SYNONYMY

Synonymy is evident in the euphemisms that are employed in reference to objects and acts that are culturally regarded as sensitive or taboo and overtly unmentionable. Examples are the many synonyms for toilet facilities *(bathroom, restroom, powder room, convenience, comfort station, lavatory, WC, latrine, john, head, pottie)* and body parts *(buttocks, butt, ass, behind, rear end, buns, keyster, derrière, tush, tukus)*. The lexical items in these lists are *synonyms* because a single conceptual category is symbolized by all the forms in each set. This is not to maintain that all the forms listed together have exactly the same meaning. Although they designate the same conceptual category, the terms in these lists have slightly

different connotative meanings. Synonyms are lexical items that are phonologically distinct but share the same designative or conceptual meaning. Connotative meaning is a topic that we will return to in Section III.

Polysemy

Polysemy must be defined in relation to *homonymy*. Homonymy is a relationship between two or more lexical items; homonymous lexical items have the same form but totally different meanings (e.g., *fan*, "device for circulating air" versus *fan*, "sports enthusiast"; *pop*, "sweet, carbonated beverage" versus *pop*, "father"). Polysemy is a relationship between the several conceptual meanings of a single lexical item; a polysemous lexical item has a single form but multiple related meanings. The polysemy of lexical items in taxonomic classification systems can be specified very explicitly. (As we shall see, this is also true in other types of classification systems.) The several meanings associated with lexical items that are polysemous in a taxonomic structure are always taxa related by direct hierarchical inclusion (Kay 1971). We have already seen an example of this in Figure 1; the lexical item *plant* has two senses, a more general one in which it refers to the entire botanical domain and a more specific one in which it refers to herbaceous plants in contrast to other kinds of plants. Another often-cited example is the English lexical item *man* (cf. Frake 1961). The inclusion relations among the three distinct categories associated with the form *man* are shown in Figure 2. The three mutually exclusive categories are related in a chain of immediate precedence. The widest category (*man* in the sense "human being") immediately includes a narrower category (*man* in the sense "male human being" as opposed to *woman*, meaning "female human being"), which in turn immediately includes a still narrower category (*man* in the sense "adult male human being" as opposed to *boy*, "nonadult male human being").

Covert Categories

In addition to many-to-one synonymy relations and one-to-many polysemy relations, two other relations between conceptual categories and their phonological representation are logically possible. The first of these is the possibility of semantic categories occurring without phonological representation. Categories of this sort occur empirically and are termed *covert categories*. One category that is often covert is the domain category itself (also termed the *unique beginner*, or *head taxon*), which includes directly or indirectly all the other categories in the classification system. When ethnobotanical and ethnozoological taxonomies lack forms

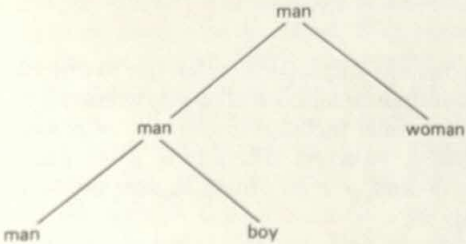

FIGURE 2. English Lexical Item *man*.

corresponding to English *plant* and *animal,* as they very often do, the existence of these covert domain categories must be established on the basis of indirect evidence, such as Berlin utilizes in establishing the Aguaruna "plant" category. The second logical possibility is that of phonological representations occurring without semantic categories. No such representations occur: although meanings exist for which there are no associated forms, forms do not exist for which there are no associated meanings. We will return to this point subsequently in considering linguistic relativity and universality.

THE LEXEME

It is apparent then that semantic categories are not isomorphic with their phonological manifestations and that the structure of taxonomic systems is defined by contrast among categories and not manifestations. It is nonetheless the case that the form of lexical items is relevant to an understanding of folk classification systems. Lexical items in general, as was stated in Section I, not only encode meaning-form correspondences, but also function as units in the grammatical, or syntactic, structure of language. In studying classification systems, anthropologists utilize a specific lexicogrammatical concept, the *lexeme.* Whether or not a grammatical unit (or combination of units) is a lexeme depends on whether its meaning can be determined from its component parts. It is a lexeme if it is semantically opaque or exocentric (outwardly oriented), such that its meaning is not determinable from its constituent parts; it is a nonlexemic compound or phrase if it is semantically transparent or endocentric (inwardly oriented), such that its meaning can be determined from its constituents (cf. Conklin 1962). This distinction is illustrated by the following pairs of lexemic and nonlexemic forms: *blackbird* (particular species of bird) versus *black bird* (a bird that is black in color); *eggplant* versus *leafy plant; night owl* versus *sleeping owl; bookworm* versus *book reader; white oak* versus *ancient oak;* and *grandmother* versus *old mother.*

Among lexemic forms, several subtypes are distinguished. Primary lexemes are either simple or complex. Simple primary lexemes consist of only a single unanalyzable constituent—for example, *bird, plant, owl, book, oak* and *mother*—whereas complex primary lexemes have two sorts of analyzable constituent structures. Unproductive complex primary lexemes have no components that refer to superordinate categories (for example, *night owl* does not refer to a kind of owl, *bookworm* does not designate book-eating worms, and *grandmother* is not a term for mothers who are grand or large), and productive complex primary lexemes, which do have constituent parts referring to superordinate categories, occur in contrast sets with simple and complex unproductive primary lexemes (for example, *planetree* refers to a kind of tree but ocurrs in contrast with *oak, maple, pine,* etc., and *eggplant* designates a plant category but one that contrasts with *squash, cucumber, avocado,* etc.). Secondary lexemes are distinct from all types of primary lexemes. They have constituents that refer to superordinate categories, and they occur in contrast sets with other categories, each sharing the same superordinate marker (for example, *white oak, post oak, red oak, jack oak, scrub oak, live oak,* and the other members of the *oak* contrast set). The taxonomic relations among these kinds of lexemes are shown in Figure 3.

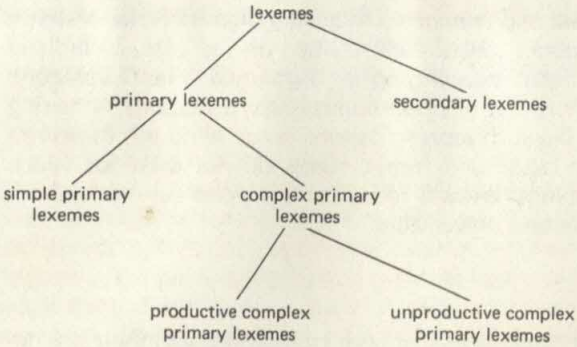

FIGURE 3. Kinds of Lexemes.

RANK IN TAXONOMIC SYSTEMS

As Berlin shows in his article, data from Aguaruna Jivaro, as well as from many other societies, support the hypothesis that a concept of rank is fundamental in ethnobiological classification systems. Aguaruna plant taxa are grouped into six mutually exclusive hierarchic ranks, which may be termed kingdom, life-form, intermediate, generic, specific, and varietal. Taxonomic and nomenclatural characteristics are the principal criteria by means of which taxa are assigned to ranks, although biological and psychological considerations also play a part. The kingdom taxon is the all-inclusive unique beginner, or head taxon, the taxon that delimits the domain. It occurs at the highest level, Level 0, in taxonomic hierarchies and, as we have seen, is very often a covert category (see Figure 4).

Life-form taxa are the next most inclusive categories in ethnobiological taxonomies. They occur only at Level 1 in taxonomic hierarchies. The lexemes for taxa of life-form rank are primary lexemes, nearly always simple primary lexemes.

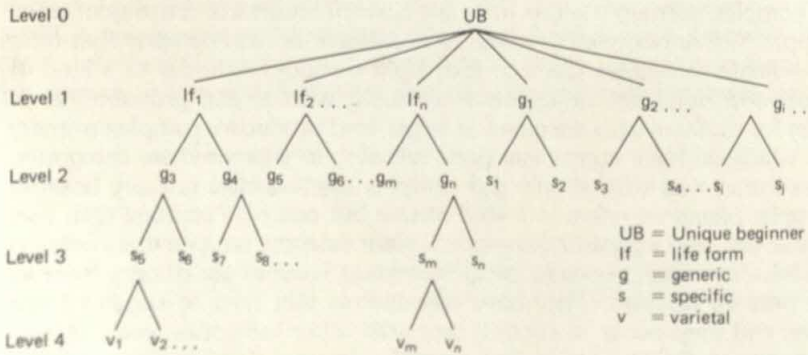

FIGURE 4. Ethnobotanical Ranks. (Source: Berlin, Breedlove, and Raven 1973, p. 215.)

Life-form taxa are few in number (0–5) and they include directly or indirectly the majority of taxa in the domain. English life-form taxa are *tree, bush, plant, grass,* and *vine.*

Taxa of generic rank comprise the core of ethnobiological taxonomies. By far the largest number of taxa in these classification systems are of generic rank. They are psychologically extremely salient: they are learned very early by children, and they are readily elicited from informants. The lexemes for generic taxa are generally primary lexemes, most often simple primary lexemes, and are more frequently used than the lexemes for taxa of other ranks. English examples are *oak, maple, pine, elm, spruce,* and so on. The majority of generic taxa occur at Level 2 and are included in life-form taxa; a few, however, are *unaffiliated generics* that occur at Level 1 and are directly included in the kingdom taxon, and others are *ambiguously affiliated generics* that sometimes are included in one life-form taxon and sometimes in another. The important point to note in Berlin's discussion of these unaffiliated and ambiguously affiliated generic taxa is that they are almost all either cultivated plants of some cultural importance or plants that are highly distinctive because of some unusual morphological feature, such as unique leaf shape, stem color, or the like.

Most generic taxa are *monotypic*—that is, they include only one type and, as such, are terminal taxa. *Polytypic* generic taxa, which dominate additional contrasting taxa, always immediately include taxa of specific rank at Level 3 (or Level 2 if the dominating taxon is an unaffiliated generic), which in turn include taxa of varietal rank at Level 4 (or Level 3 if the indirectly dominating generic taxon is unaffiliated). *Specific taxa* are few in number and *varietal taxa* are rare. The lexemes for both are binomial (two-name) secondary lexemes, examples of which are *white oak, post oak, red oak, jack oak,* and *scrub oak.*

Taxa of intermediate rank (the sixth rank as defined by Berlin) are categories of a special type. They are mid-level classes, occurring most commonly between Level 1 life-form and Level 2 generic taxa, but also between taxa of any other adjacent ranks. As Dougherty points out in Chapter 8, mid-level groupings of *trees* into "leaf-bearing," "needle-bearing," and "frond-bearing" are examples of covert intermediate taxa in our ethnobotanical classification system. For many Americans, the "needle-bearing" intermediate category (*pine, spruce,* and *fir*) is not covert but rather is recognized overtly as *evergreens.* Mid-level categories occur in most taxonomic systems. They are, as Dougherty indicates, quasi-categories or temporary distinctions whose importance lies in the fact that they reflect the progress of ongoing change. This is a topic to which we will return in Section IV.

At the outset of this introduction, it was stated that attention would be focused on taxonomic classification systems because they are the type of system about which anthropologists have learned the most and because an understanding of taxonomic systems provides a basis for understanding less thoroughly studied types of systems. A number of types of semantic contrast in addition to inclusion serve as the means of ordering classification systems. We turn now to four types of nontaxonomic systems: constituent classifications, functional classifications, contingency classifications, and network classifications (cf. Tyler 1978 for a recent, more detailed discussion of semantic relations and the ordering of classification systems).

OTHER TYPES OF CLASSIFICATION SYSTEMS

Constituent, or *partonomic,* classifications are systems built on the basis of *constituent* semantic relations. Contrastive categories in this kind of classification system are related in the following ways: category X may be a part of, a member of, or an ingredient in category Y, in which case Y is the whole of which X is a part, the team of which X is a member, or the recipe in which X is an ingredient (cf. Frake 1964, Casagrande and Hale 1967, Spradley 1979). The most thoroughly studied constituent classifications are ethnoanatomical systems (cf. Brown 1976, McClure 1976), which, like taxonomies, often involve several hierarchical levels (*knuckle* is a part of *finger,* which is a part of *hand,* which is a part of *body;* and *wrist, forearm, elbow,* and *biceps* are parts of *arm,* which is part of *body*). The constituent relation "member of" has to date received little attention, but examples include the *family* category, which has among its members *father, son,* and *brother,* and the *baseball team* category, which has the members *pitcher, shortstop,* and *manager,* among others. Examples of the ingredient relation include *spice* as an ingredient in *yeast,* which is an ingredient in *beer,* and *chili peppers,* an ingredient in *chili powder,* which is an ingredient in *chili.*

Functional classifications are constructed on the basis of the *instrumental* semantic relation. Contrastive categories in this kind of classification are related as between the object or force and the action or process it is used to perform or accomplish: X is the means of doing Y, and Y is what is done through the aid or application of X. Examples include *scissors–cut, beer–drink, key–unlock,* and *fist–punch.* Leach's (1964) celebrated classification of "familiar English animals" is a functional classification: *livestock* are domesticated animals used for *food,* and *pets* are domesticated animals *not* used for *food; game* are nondomesticated animals used for *food,* and *wild animals* are nondomesticated animals *not* used for *food.* Functional categories of *shoes* are another example: a *sports shoe* is a shoe used for sporting activities, a *running shoe* is used for running, and a *tennis shoe* is used for playing tennis.

Contingency classifications are formed on the basis of *conditional* semantic relations. Contrastive categories are related as cause and effect or result, as prerequisite and outcome, as mutual concomitants, and as stages in a process: X is usually or necessarily followed or accompanied by Y, and Y is the usual or necessary consequence or concomitant of X. Cause-effect relations are extensively involved in disease classification systems: e.g., *bad weather* causes *infection,* which causes *fever, runny nose, sore throat,* and *antibodies;* and *contagiousness* is the result of *germs* (cf. D'Andrade 1976, p. 175). *Money* is a prerequisite for *robbery* (where there is no money, robbery is not a possible outcome), *sleep* is a necessary requirement for *dreaming* (except in the case of *daydreaming*), and *drunkenness* is an outcome of *alcohol* consumption (in addition to also being caused by it) (cf. Casagrande and Hale 1967, Agar 1975). *Smoke* is a concomitant of *fire,* and *violence* is generally accompanied by *bloodshed.* Life stages illustrate stage–process relations: *childhood* precedes *adulthood* and *middle age* precedes *old age* in the life process. In Chapter 5, the principal concern of Michael Agar is to show that *getting-off* (injecting heroin), a central event in the culture of *street junkies,* is conceptually organized as a process comprised of an ordered set of stages.

Network classifications are formed not on the basis of a single type of semantic

relation but rather by complex interlinkages involving all types of relations (cf. Frake 1964, Werner and Topper 1976). Agar's analysis of the set of lexical items semantically related to *getting-off* in the argot of heroin addicts also illustrates this type of classification system. Stage-process relations are at the core of this system, but a number of other semantic relations are also shown to interlink contrastive categories in this domain. These include, in addition to inclusion, constituent, instrumental, and contingency relations, a number of other relations that interconnect the core with other categories or category sets. The network of categories surrounding *getting-off* includes categories linked by spatial relations (*shooting gallery,* locus of or location for *getting-off*), agentive relations (*junkie,* doer of *getting-off*), and several others. All the semantic relations that occur in classification systems are not exemplified in the *getting-off* system (e.g., comparative relations, attributive relations), but the major ones are represented. Additional examples of semantic relations in network classifications can be expected to emerge as research progresses in this frontier area.

COMPONENTIAL ANALYSIS OF LEXICAL MEANING

To this point, discussion has been concerned with relations among categories in semantic structures. The types of semantic contrast that differentiate conceptual categories in classification systems have been described, as have the relations between categories and lexemes. It remains now to discuss the analysis of lexical meaning.

Lexical items, as we have seen, encode a correspondence between abstract properties of sound and meaning. The meaning of lexical items derives from attributes or properties of entities in the nonlinguistic world of experience and imagination. These properties of referent entities are encoded in lexical items as conceptual abstractions, which are generally termed *semantic features,* or *components.* There are three senses in which lexemes refer: they denote referent entities or *denotata* (singular, *denotatum*); they designate categories of entities or *designata* (singular, *designatum*); and they signify attributes or *significata* (singular, *significatum*) of the entity membership of categories.

Componential analysis, which is a procedure for determining the meaning of the lexemes in a folk classification system, is aimed at discovering the semantic components or features (significata) that constitute the distinctive meaning of each lexeme in the system. Not all attributes of referent entities are encoded in lexemes as components of lexical meaning, only those that are necessary and sufficient—that is, only those that actually differentiate the meanings of the lexemes in the classification system. These criterial, or distinctive, features are the means by which referent entities are recognized and classed as members of categories, the means by which categories are differentiated in semantic structures, and the means by which the distinctive meanings of the lexemes in the classification system are defined. Definitions of the meaning of lexemes as sets or bundles of distinctive semantic components are known as *componential definitions.*

We return (with apologies to Frake) to our earlier *hamburger–cheeseburger* example to illustrate componential analysis and componential definitions. *Chowder* and *hamburger* are contrastive categories that are semantically related only in that they are both categories of food—that is, they are both members of a

single semantic domain. The categories *hamburger* and *cheeseburger,* on the other hand, are very closely related semantic categories. They are distinguished from such other categories in the domain as *sandwich* and *hot dog* by the presence of a bun and a patty, and they are distinguished from each other only by the presence or absence of a slice of cheese. Presence of cheese and absence of cheese are the criterial, or distinctive, features that distinguish *hamburger* from *cheeseburger.* Whether or not an object is a *hamburger* as opposed to a *cheeseburger* cannot be determined from the presence or absence of a slice of tomato, pickles, onions, lettuce, or sesame seeds on the bun. These are noncriterial, or connotative, features that do not differentiate between the *hamburger* and *cheeseburger* categories and have no bearing on the classification of an object as one or the other. Although noncriterial features are therefore irrelevant in the componential analysis of lexical meaning, they are important in the analysis of transferred meaning, as will be shown in Section III.

The lexical meanings of *hamburger* and *cheeseburger* are defined by distinctive features on several dimensions of semantic contrast. They contrast with *sandwich* and *hot dog* on dimensions involving presence or absence of buns and presence or absence of a meat patty. Semantic features on the bun dimension can be symbolized ±BUN and those on the patty dimension ±PATTY. They contrast with each other on a dimension involving presence or absence of a slice of cheese. The features on this dimension can be symbolized as ±CHEESE. When the domain feature—the feature shared by all categories in a classification system, which in this case is +FOOD—is included, the componential definition of *hamburger* is +FOOD, +BUN, +PATTY, −CHEESE; that of *cheeseburger* is +FOOD, +BUN, +PATTY, +CHEESE.

Dimensions of Contrast

Semantic features occur on several types of dimensions of contrast. There are five that need to be described: binary contrast dimensions, multiple contrast dimensions, ranked contrast dimensions, polar contrast dimensions, and relative contrast dimensions (here we follow Leech 1974, pp. 95–125). There is as yet no standard notation for representing semantic features and dimensions. Notational systems differ widely, but because they are all simply equivalent means of representing semantic structures, there is little reason for preferring one type of notation over another. A notational system (again borrowed from Leech 1974), in which upper-case letters are used for features and features are preceded by symbols indicating the type of dimension on which they occur, is used in the section introductions in this volume because it is easy to read. Other notational systems will be explained when their interpretation is not immediately apparent.

The most commonly occurring type of dimension is the *binary contrast* dimension. Contrasts on this type of dimension are absolute. Two opposed categories are distinguished in a strictly yes/no, either/or fashion. There are no fuzzy boundaries. As just shown, the dimension of contrast that distinguishes *hamburger* and *cheeseburger* allows only two possibilities: presence or absence of cheese. This binary contrast is symbolized by the features +CHEESE and −CHEESE, the plus sign indicating the presence of the attribute and the minus sign indicating its absence. Sex is another binary contrast dimension. On the basis of sex, an organism can be classed as only +MALE (male) or −MALE (female). (The use of MALE rather than FEMALE to indicate features on this dimension is

conventional, albeit undoubtedly sexist.) And life-state is another: an organism can be only +LIVE (alive) or −LIVE (dead), there are no other possibilities.

A second closely related type of dimension is the *multiple contrast* dimension. Contrasts on this type of dimension are also absolute, but they involve more than two opposed categories. Sets of contrasting categories—that is, *contrast sets*—share all but one distinctive feature, and each category in the set is distinguished by a different feature on a single multiple contrast dimension. The categories in a contrast set may, for instance, be distinguished only by shape, one category being distinguished by the feature *SHAPE (round), a second by the feature @SHAPE (elongated), a third by the feature #SHAPE (elliptical), and so on. Texture and color are other attributes of referent entities that occur as distinctive features on multiple contrast dimensions. The symbols marking features on multiple contrast dimensions are assigned arbitrarily.

The third type of dimension of contrast is the *ranked contrast* dimension. On this type of dimension, contrasts are absolute, as they are on the binary and multiple contrast dimensions, but they are also ordered. Life-stage categories are examples of categories distinguished on a ranked contrast dimension. The category *infancy* has the feature 1STAGE, the category *childhood* has the feature 2STAGE, the category *adulthood* has the feature 3STAGE, etc. The generation dimension in calculating kinship distances also has rank-ordered features. Kinsmen are classed in categories distinguished by ordered features, for example, >GEN (ascending generation), =GEN (same generation), and <GEN (descending generation). The ordering of features on ranked contrast dimensions is indicated by quantification symbols.

The three types of dimensions of contrast discussed thus far differentiate among categories on the basis of absolute attributes. On these dimensions, features refer to attributes, or properties, of the entities classed in contrasting categories. Two other dimensions, the polar contrast dimension and the relational contrast dimension, distinguish categories on the basis of relative attributes. Features on these dimensions refer not to attributes or properties of entities but rather to relations between entities and norms or between entities and other entities (for absolute versus relative attributes, see Bruner, Goodnow, and Austin 1956, also Kronenfeld 1974).

On *polar contrast* dimensions, opposed categories are distinguished relative to the norm for the entities classed in the categories. Size is an example of a polar contrast dimension. Opposed categories are distinguished as ↑SIZE (large) or ↓SIZE (small) in relation to the size norms for the objects in question. A watermelon that is classed in a category distinguished by the feature ↓SIZE is small only with respect to watermelon norms and is, in fact, larger than a radish that is classed in a category distinguished as ↑SIZE. Other examples of polar contrast dimensions are age, which has the features ↑AGE (old) and ↓AGE (young), and wealth, which has the features ↑WEALTH (rich) and ↓WEALTH (poor).

Relational contrast dimensions distinguish between contrasting categories on the basis of opposed relations between two entities. Features differentiate between a relation in which the two entities are linked X-to-Y and an opposite one in which they are connected Y-to-X. One example is the buy/sell relational contrast dimension. Categories such as *buyer* and *seller* that are the same in all other features are distinguished on this dimension by the features ←BUY (X buy Y)

and →BUY (Y buy X). The categories *dealer* and *junkie* in the argot of urban heroin addicts are also distinguished by these features. Relative age is another relational contrast dimension: *big brother* and *little brother* are distinguished only by the features →OLDER (elder) and ←OLDER (younger). Other relations between referent entities that occur as distinctive features on relational contrast dimensions are teach/learn (*teacher* is distinguished as →TEACH and *pupil* as ←TEACH), rent/lease (*landlord* is distinguished as →RENT and *tenant* as ←RENT), and parent/child (*parent* is distinguished as →PARENT and *child* as ←PARENT).

Markedness

The notion of *markedness* is basic in componential analysis, as well as in other areas of research that will be encountered in subsequent sections of this book. In marked-unmarked oppositions, unmarked members are more general, more salient, or more frequent in occurrence than the marked members with which they contrast. In the opposition *hamburger/cheeseburger, hamburger* is the unmarked member and *cheeseburger* is the marked member. *Cheeseburger* is distinguished by a feature specifying the presence of an attribute (+CHEESE) as opposed to *hamburger,* which is either distinguished by a feature indicating the absence of the attribute (−CHEESE) or is neutral with respect to the presence or absence of the attribute. *Hamburger* thus has two senses: a contrastive specific sense in which it indicates the opposite of *cheeseburger,* and a neutral general sense in which it specifies the superordinate category dominating both *hamburger* and *cheeseburger.*

Relative as well as attribute features occur in marked-unmarked oppositions. The adjectives *long* and *short,* for example, are distinguished on the length dimension. *Short* is the marked member of the opposition and specifies the contrastive sense, short relative to the norm (↓LENGTH); *long* is the unmarked member and indicates both the contrastive sense, long relative to the norm (↑LENGTH) and the neutral sense in which the length dimension is specified but the contrast is not marked. The contrastive senses of *long* and *short* are illustrated in the sentences, "The road is very long" and "The road is very short," while the neutralized sense of *long* occurs in the sentence, "The road is 26 miles long." Notice that the length dimension cannot be indicated by the marked term: "The road is 26 miles short" is not a possible sentence in English.

In Chapter 6, Geoffrey Leech presents a componential analysis of a complex set of lexical items. His discussion very clearly exemplifies the concepts and procedures of componential analysis in an analysis of kinship semantics, a cultural domain that is of central importance in cognitive anthropology. The concepts and methods of componential analysis have been applied to many other domains, but they have been most thoroughly explored in studies of kinship terminologies, most particularly in studies of the American English kinship terminology. An important point revealed in these studies is that componential analysis is not a mechanical procedure that can be applied automatically to any set of data. That different investigators will not always arrive at the same analysis of a particular terminological system has troubled cognitive anthropologists since the publication in the early 1960s of two componential analyses of the American English kinship terminology, the first by Anthony F. C. Wallace and John Atkins (1960)

and the second by A. Kimball Romney and Roy G. D'Andrade (1964). One issue that has concerned cognitive anthropologists confronted with alternative solutions that provide equally accurate accounts of the same data and do not differ as the result of methodological inadequacies is the problem of psychological reality or cognitive saliency—of determining which of the two analyses best represents the cognitive structure actually employed by society members. To decide issues of psychological reality, investigators turn to independent psychological tests and measures (examples will be found in many of the following chapters (for example, Chapter 8, but see particularly Chapter 13). Another issue is that alternative solutions may both be psychologically real, in which case the two models either may vary among individual society members or they may be shared by all society members. The problem of alternative solutions is a topic we will take up in Section IV.

RELATIVITY AND UNIVERSALITY

Leech's discussion is also noteworthy because he presents his componential analysis, as well as his consideration of color terminologies, with the aim of exploring the nature of semantic universals. This reflects an important direction of current research in cognitive anthropology. Until quite recently, research into the nature of cognitive structures and processes had been dominated by the Sapir-Whorf hypothesis, named after Edward Sapir and Benjamin Lee Whorf, two American linguistic anthropologists of the 1920s and 1930s who argued for what has come to be regarded as an extreme form of linguistic and cultural relativity. According to Whorf (1956, p. 213),

> the categories and types that we isolate from the world of phenomena we do not find there because they stare every observer in the face; on the contrary the world is presented in a kaleidoscopic flux of impressions which has to be organized in our minds—and that means largely by the linguistic system in our minds. We cut nature up, organize it into concepts, and ascribe significance as we do, largely because we are partners to an agreement to organize it in this way—an agreement that holds throughout our speech community and is codified in the patterns of our language.

This often-quoted passage (as well as others from Whorf's writings) has generally been interpreted as a statement of extreme or complete relativism. Although Whorf's views, as we will see shortly, were actually not as extreme as they were represented to be, the highly influential position that came to be associated with his name is that cognitive categories are not formed in response to divisions in the natural world or to universal properties of the human mind, but rather in response to the organization of grammatical (including lexical) systems. And because grammatical systems are determined by arbitrary agreements in speech communities, the cognitive systems created by grammatical systems are entirely relative to, in fact idiosyncratic to, speech communities. This position, that cognitive systems are completely relative to particular communities, is also evident in this statement by Sapir (1931:578):

> Inasmuch as languages differ very widely in their systematization of fundamental concepts, they tend to be only loosely equivalent to each other as symbolic devices and are, as a matter of fact, incommensurable in the sense in which two systems of

> points in a plane are, on the whole, incommensurable to each other if they are plotted out with reference to differing systems of coordinates.

If languages are incommensurable—that is, if they have no common measure—they they cannot be compared, and translation is ultimately impossible (cf. Lounsbury 1969).

This position contrasts with another position that has emerged in connection with recent research, the *limited relativist,* or *universalist,* position. As Floyd G. Lounsbury states, the emphasis of this position is on the psychic unity of humankind and the commonality of human experience; variability in language and culture is acknowledged, but the focus of research is the discovery of ultimate prime concepts—the fundamental and universal concepts underlying the variable superstructures of linguistic and cultural systems (1969, p. 10). The discovery of ultimate concepts provides a basis for the commensurability, comparability, and translatability that is denied by proponents of the complete relativist position. So the limited relativist position is universalist in its emphasis on ultimate primes, yet relativist in its recognition of variability in all aspects of linguistic and cultural systems above ultimate primes.

As Leech points out, several theoretical distinctions are basic to the study of semantic universals. One is Chomsky's distinction between formal and substantive universals. *Formal universals* are universal characteristics of language form (for example, it may be postulated that the meaning of lexemes in all languages can be defined in terms of sets of distinctive features, or components), and *substantive universals* are universal characteristics of language content (it may, for example, be postulated that the features +HUMAN and −HUMAN occur in all languages). The second theoretical distinction that is needed is that between strong and weak substantive universals. The former are universal characteristics that occur in all languages, whereas the latter are characteristics that occur in universal sets from which all languages draw a subset. The recent research of cognitive anthropologists has been concerned primarily with discovering substantive universals of the latter variety.

Color Classification

An area in which substantive universals have been extensively studied is color classification. The impetus for this universalist research was provided, as Leech points out, by Berlin and Kay's book, *Basic Color Terms: Their Universality and Evolution* (1969), in which it is concluded that there is a substantive universal set of eleven focal color categories that are encoded in *basic color terms* (for example, English *black, white, red, green, yellow, blue, brown, purple, pink, orange,* and *grey*) in the languages of the world. These are ultimate prime concepts. Although languages vary in the number of color categories they encode (from as few as two to the full set of eleven), in all cases the foci of these categories are located at the same points in the color space (defined by the dimensions hue, brightness, and saturation). Berlin and Kay also conclude that there is a universal sequence of seven stages in which the eleven focal color categories are encoded in specific languages: the first focal colors encoded are black and white (Stage I), the third is always red (Stage II), the fourth and fifth are green and yellow, in either order (Stages III and IV), the sixth is blue (Stage V), the seventh is brown (Stage VI), and the eighth through eleventh are purple, pink, orange and grey (Stage VII). As we will see, this sequence

has been revised by Kay (Chapter 14) to take into account color term data acquired since 1969.

The research on universal color categories and their encoding sequence has been used as a basis for arguing that the complete relativist position should be rejected in favor of the universalist, or limited relativist, position. It has also been widely argued that semantic universals in color classifications are based on underlying innate faculties of the human mind, or neurophysiological processes in human perception, and that, therefore, rather than language exerting an influence over thought, as maintained by the Sapir-Whorf hypothesis, it is thought that influences language (cf. Witkowski and Brown 1977 and Kay and McDaniel 1978). In Chapter 7, John A. Lucy and Richard A. Shweder take up this issue and, on the basis of new evidence, argue that Whorf's conception of the relation between language and thought is still more accurate and acceptable than any competing view.

Lucy and Shweder review the color research relating to the language-and-thought question, focusing on two traditions of recognition memory research: One emphasizes focality and maintains that focal colors have greater memorability than nonfocal colors because they are perceptually more salient; the other emphasizes codability (the relative ease with which different colors are linguistically encoded) and maintains that high-codability colors have greater memorability than low-codability colors because linguistic encodings are the vehicles of memory. Focality and codability were never directly contrasted as predictors of recognition memory in either research tradition, so Lucy and Shweder conducted a series of five experiments to investigate interrelations among the three variables: focality, codability, and memorability. The major finding they report is that *communicative accuracy,* a measure of codability, is a much more accurate predictor of color memory in both short- and long-term tasks than is focality. This indicates, Lucy and Shweder argue, that language is used in memory—that the linguistic encodings they refer to as *basic color descriptions* (conventional constructions such as *light green, military green, pea-soup green, light royal blue, greenish turquoise blue,* and *deep-sea blue,* as well as basic color terms such as *green* and *blue*) are the principal mechanisms and means of color memory, and that focality is not at all predictive when linguistic codability is taken into consideration.

Lucy and Shweder conclude that Whorf's original formulation of the relation between language and thought is in close accord with these findings, particularly when a misconception about Whorf's view of linguistic relativity is corrected. As Lucy and Shweder point out, the Sapir-Whorf hypothesis that thought is shaped completely by language is a misrepresentation of Whorf's actual position. Whorf, in fact, did not deny that there are sublinguistic universals that influence language and culture; he argued that the influence of these biological universals is a matter of "appropriation": language operates independently of sublinguistic universals, sometimes encoding them and sometimes ignoring them. So focal colors exist but may or may not be appropriated linguistically, as witness all the languages that lack basic color terms for one to nine of the eleven universal focal colors; thus, focality does not exert any direct or complete influence over language. The principal direction of influence seems to be, as Whorf argued, language to thought: basic color descriptions act as the principal vechicles for color memory.

Ethnobiological Rank and Relativity

The final chapter in this section deals with another variety of universals. In Chapter 8, J. W. M. Dougherty is concerned with the universality and relativity of hierarchic ranks in taxonomic classification systems. As will be recalled from our earlier discussion, Berlin argues that taxa in ethnobiological classification systems are always grouped into six mutually exclusive hierarchic ranks, and that generic rank taxa are always the psychologically most salient categories in these systems. Dougherty does not question the universality of ethnobiological ranks but argues that relativity is importantly involved in the determination of which rank is the most salient or basic in any particular ethnobiological classification system.

The psychological saliency of the categories in a domain is determined from behavioral data. The basic-level categories in a classification system are those that are most frequently used in everyday interaction, most easily recalled by informants, identified by holistic or gestalt perception, and earliest in ontogenetic development. Dougherty's concern is to show that while taxa of generic rank are most salient in the ethnobiological classification systems of people who interact frequently with their biological environment and whose subsistence depends directly on it, categories superordinate to the generic level (that is, taxa of life-form and intermediate rank) are most salient in the systems of people who do not maintain a high degree of interaction with their biological environment. The thrust of Dougherty's discussion is to emphasize the limited relativity of the most salient or basic rank in taxonomic classification systems. Fundamental categories are not fixed by innate predispositions, by linguistic structures, or by objective reality; rather they vary with the degree to which society members interact with the entities classed in the domain. In societies in which members are highly involved with the domain, the generic rank is basic, but in societies in which members are unconcerned with the domain, suprageneric ranks are basic. So although the six ethnobiological ranks are (so far as is now known) universal, which of them is most basic in a taxonomic classification system is determined relative to the overall salience, or cultural significance, attached to the domain by society members.

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4

The Concept of Rank in Ethnobiological Classification: Some Evidence from Aguaruna Folk Botany

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Over the last several years, my colleagues and I have attempted to provide some initial hypotheses concerning the nature of folk biological classification and nomenclature (Berlin, Breedlove, and Raven 1973, 1974; Berlin 1972, 1973; Raven, Berlin, and Breedlove 1971). We have suggested that classes of plants and animals are grouped into a small number of hierarchic ranks such that taxa within each rank are mutually exclusive and exhibit essentially comparable degrees of differentiation from one another. We claim that taxa of the same rank exhibit nomenclatural, biological, taxonomic, and psychological characteristics which permit the assignment of any particular taxon to some particular rank in most instances.

Recently, several folk biologists have made some criticisms and voiced certain doubts about the nature of folk biological rank—suggesting, for example, that the boundaries between ranks are arbitrary (Hunn 1973, 1976) or, more seriously, that the concept itself has no validity (Bulmer 1974; Randall 1976). Other colleagues have pointed out informally that the criteria for establishing the rank of particular taxon are not explicit and that writing on this subject has been unclear (Kay, personal communication; Conklin, personal communication).

This report attempts to deal with some of these criticisms by reviewing from a somewhat different perspective the basic outlines of our original hypotheses in light of some new data on Aguaruna botanical classification.[1] The report will conclude that certain aspects of our understanding of folk biological classification must be changed from that presented in 1973, but that, in the main, the Aguaruna data conform to the essence of that description. I hope that the revised account presented here will contribute to the development of an empirically based, cross-culturally valid typological description of ethnobiological classification.

SEMANTIC CONTRAST, TAXONOMIC LEVEL, AND RANK: SOME PRELIMINARIES

One of the major contributions of Kay's elegant explication of folk taxonomic structure was an explicit definition of formal semantic contrast (Kay 1971). Of the four types of semantic contrast which can be defined by relations of immediate precedence, one, that of *direct contrast,* is of particular interest in terms of my discussion of rank. For Kay, any two taxa are in direct contrast if they are immediately preceded by the same taxon. Thus, taxa marked by the names *oak* and *maple* are in direct

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† Source: *American Ethnologist* 3:381–399, 1976.

contrast in that they are both immediately preceded by a taxon labeled by the word *tree* (in English folk botany). *Red maple* and *sugar maple* are both immediately included in *maple* and, as a consequence, are also in direct contrast. Finally, for most speakers of English, *tree* and *bush* label taxa which are in direct contrast in that both are included in a taxon labeled by the word *plant.*

Kay's definition of direct contrast had an interesting empirical consequence in that it led him to formalize the concept of *contrast set,* a name for a notion that had been used in cognitive anthropological literature with various and sometimes conflicting meanings for several years. Formally, a contrast set is comprised of just those taxa which are immediately preceded by the same taxon.

In addition to these strictly formal characteristics, it could be claimed on introspective grounds (grounds which can be given empirical validation) that some substantive generalizations can be made about the taxa which comprise, in our examples, the contrast sets {oak, maple, . . .}, {sugar maple, red maple, . . .} and {tree, bush, . . .}. Speaking very loosely, each set contains taxa which exhibit similar or comparable degrees of internal variation. That is, the "degree of separation" (Simpson 1961:191) between the members of each set, as well as the amount of divergence among the organisms included in the taxa of each respective set, is roughly equivalent. Thus, in the above examples, *formal* semantic contrast corresponds well with our informal intuitions about the substantive nature of the taxa which participate in such contrast.

Unfortunately, in folk biological classification, members of the same contrast set often do not exhibit the same degree of internal variation. In Aguaruna folk botany, for example, there are at least fifty-seven formally defined first level taxa (i.e., those taxa immediately included in the covert taxon 'plant'). One taxon, *númi* 'tree,' is polytypic and includes more than 275 named subordinate members which range over more than 400 botanical species. At the other extreme, thirty-three first level taxa are monotypic and often refer to but a single species, e.g., the large bamboo, *kéŋku* (*Bambusa* sp.).

Glick's characterization of the folk botanical classification of the Gimi of New Guinea is quite analogous to the Aguaruna. He notes,

> there are more than twenty [first level] botanical categories, ranging in size from *da* 'tree' with at least 200 members, through *koi* 'ginger' with four, and on down to several problematical sets containing only two or three members apiece (1964:274).

Comparable data are not hard to come by in the domain of folk zoology, as can be seen in the descriptions of Cantonese ethnoichthyology (Anderson 1967), Kalam folk zoology (Bulmer 1968; Bulmer and Tyler 1968), and Tzeltal animal classification (Hunn 1973). An example from the latter's work illustrates the direct contrast relation between the first level taxon, *mut* 'bird,' with more than 100 subordinate taxa, and *soc'* 'bat,' a simple, monotypic class. Hunn cogently describes the situation as follows:

> One would like to say that *soc'* 'bat' (level 1) *contrasts with* categories such as *heš* 'jay' (level 2) rather than with *mut,* which by Kay's definition belongs

> to the same 'contrast set.' In fine, taxonomic structures defined solely in terms of the relation of set inclusion cannot adequately account for the notion of contrast, relevant to folk as well as scientific classification, which relates taxa to one another by reference to degree of differentiation they exhibit. Taxonomic categories [i.e., ranks] must be defined to account for this relation (Hunn 1973:87–88).

Partly in response to some of these empirical observations on the nature of folk biological taxa of varying degrees of differentiation, Kay has recently revised his 1971 treatment—which explicitly rejected the notion of folk biological rank—and now provides us with a formalism which easily permits the incorporation of rank into folk taxonomy (Kay 1975). Nonetheless, he is quick to advise the folk biological fieldworker that "ranks must be stipulated on the basis of *empirical considerations.* They are not in general deducible from knowledge of the relations of immediate precedence" (1976:161; italics mine).

Just what "empirical considerations" are involved in the assignment of a particular taxon to some particular rank remain controversial. Bulmer, arguing strongly for the priority of taxonomically defined contrast relations, asks for criteria on how to "distinguish clearly between unaffiliated generics [such as Aguaruna *kéŋku,* Tzeltal *soc',* and Gimi *koi*] and life forms [such as Aguaruna *númi,* Tzeltal *mut,* and Gimi *da*]" (Bulmer 1974:23). Hunn, in spite of his awareness of the need to recognize folk biological rank, has apparently despaired of achieving some workable, non-*ad hoc* characterization, claiming that "boundaries between adjacent categories [i.e., ranks] of taxa are *arbitrary* . . . due to the continuous variation of taxonomic heterogeneity" (1973:106, 112; italics mine).

In spite of Hunn's and Bulmer's reservations, I believe that the data from Aguaruna indicate that taxa of differing ranks can be clearly recognized, that they do not merge arbitrarily into one another as on some undifferentiated continuum, and that their recognition is essential for a full understanding of the Aguaruna conceptual organization of the plant world.

AGUARUNA 'PLANT'

Many ethnographers are apparently willing to accept the domain of 'plant' or 'animal' as the starting point for ethnobiological investigation only if these domains can be shown to constitute a conceptual category by virtue of some habitually applied label (see Brown 1974; Conklin 1954, 1962). I believe that such a stricture is overly restrictive. As I have stressed several times, while a name may be an unambiguous indicator of a category, absence of a label does not necessarily imply the absence of a category. If one observes that 'plant' or 'animal' is named in some language, but not in others, the empirical question immediately arises, "Are there sufficient data to indicate the cognitive recognition of the category in those languages where [these categories] are not named?" (Berlin 1974:328). The answer, of course, will depend on the ethnobiological facts in any particular case.

In Aguaruna, no separate term for the domain of 'plant' has been elicited. The descriptive phrases, *númi áidau* lit., 'all trees,' *dukap áidau* lit., 'all leaves,' and *ahąkbau áidau* lit., 'all places of cultivated plants' are listed in the only published bilingual dictionary of the language (Larson 1966) with the meaning of 'plant.' *númi áidau* has been verified with one informant but does not appear to be widely shared or understood to have this meaning with those Aguaruna with whom I have worked. A variant of *dukap áidau* has been heard on occasion in natural conversation when my field ethnobotanical assistants were asked by other Aguaruna where they were going (on some particular collecting session) and responded, "We're going to look for leaves" (i.e., 'plants').

Other indirect linguistic evidence for the existence of the domain 'plant' is seen in the extensive vocabulary for morphopological features of plants and plant growth terms—all expressions which apply exclusively to organisms which are independently assigned to the plant kingdom on botanical grounds. While the presentation of a full inventory would be inappropriate due to space considerations, the following terms are illustrative.

dúka[2]	'leaf'
káŋkap	'buttress root'
susuhí	'underground root'
saép	'bark'
hií	'bud'
titíhi	'main stem above first branching'
kuihí	'terminal growing tip (of stem)'
néhe	'tuber'
numíhi	'erect stem'
sakúti	'inner node'
yaŋkuhí	'flower'
kanáwe	'branch'
híŋkayi	'seed'

On less explicit grounds, there is much informal evidence that the world of plants is recognized as a distinct domain by the Aguaruna. In collecting more than 20,000 specimens (4,000 numbers), informants, some of whom were monolingual, never selected organisms *other* than plants in actual field collecting situations; mushrooms and other fungi were not considered to fall within the domain.

Finally, my experience with the Aguaruna plant name lexicon is identical to that of Hays for the Ndambu who notes,

> in the course of discovering over one thousand lexemes which were subsequently validated as plant names, I never encountered uses of these expressions as labels for anything which I would not consider to be plants, except as they were also used to name products of these plants (e.g., *sa'tari* which may be glossed 'tree or shrub' or 'wood'). By the same token, nothing I considered to be plants, except for 'mushrooms and bracket fungi' was ever labeled with a lexeme which could not be meaningfully inserted in the elicitation frames in the same ways as could plant names (Hays 1974:148–149).

Thus, while the Aguaruna do not label the unique beginner, there is considerable evidence that the domain is conceptually recognized by these people. In this respect, theirs is similar to several other systems which have been recently studied (Berlin, Breedlove, and Raven 1974; Hays 1974; Turner 1973) and conforms to the general statement made earlier (Berlin 1972) that a label for the unique beginner in folk biological nomenclature occurs late in a language's development.

TAXA OF LIFE FORM RANK IN AGUARUNA

As in other systems of folk botanical classification, the Aguaruna bring order to the diversity of the domain of plants by classifying groups of similar organisms into a number of taxa of greater and lesser inclusiveness. The most inclusive, wide-ranging categories in folk biological taxonomies (apart from the unique beginner) have been called *life form taxa* and can be assigned to *life form rank*. The recognition of life form taxa depends on the following characteristics:

(1) Life form taxa occur at the first level of the folk taxonomy and are immediately preceded by the unique beginner when the unique beginner is defined as the kingdom 'plant' or 'animal.' Taxa of life form rank are few in absolute number, they are invariably polytypic, and they include among themselves the majority of all taxa of lesser rank.

(2) Nomenclaturally, life form taxa are labeled by primary lexemes and immediately precede taxa most of which are labeled by primary lexemes.[3]

(3) Biologically, life form taxa are diverse in extension as can be objectively measured by an enumeration of the number of distinct biological species included in each such class.

(4) Psychologically, life form taxa can be defined by a small number of biological characters, most of which refer to stem habit (in folk botany) and represent what Hunn would call *deductive categories* (Hunn 1973:94).

The data from Aguaruna uniformly conform to the foregoing characteristics. There are but four first level taxa which exhibit all of these characteristics. Among them they include 75 percent of all taxa of lesser rank. Each is labeled by a primary lexeme. All are extremely diverse in range biologically, and each can be defined by a small number of biological characters. These taxa and their respective glosses are:

núm̃i 'trees and shrubs exhibiting woody (non-pithy) stems with erect habit'

dáek 'plants exhibiting twining stem habit, including woody lianas and herbaceous vines'

dúpa 'net-leaved plants and small shrubs exhibiting herbaceous or pithy stems'

šíŋki 'palms, excluding the small reed-like (e.g., *yaún Chamaedorea* spp.) and trunkless (e.g., *takának Taenianthera* sp.) forms'[4]

Any plant which, in its adult form, consistently exhibits all of the characteristics of one of the major life form taxa will be assigned to that taxon, though there exist a substantial number of taxa of lesser rank which are excluded from these four major groupings because of their distinctive morphological characteristics (see below). Thus, while life form taxa in Aguaruna are broad ranging, they do not exhaust the plant kingdom totally. In this respect, the Aguaruna system of plant classification is identical to that of the Tzeltal (Berlin, Breedlove, and Raven 1974), the Ndambu (Hays 1974), Quechua (Brunel 1974), several languages of the American northwest (Turner n.d.), Hanunóo (Conklin 1954), and K'ekchi (Wilson 1972), and, except for the conceptual recognition of a sub-set of palms, is reminiscent of classical plant classification ('tree,' 'vine,' 'herb'). These three major groupings, it might be speculated, represent such distinct perceptual discontinuities that their recognition may constitute a substantive near-universal in prescientific man's view of the plant world.

TAXA OF GENERIC RANK

In the systems of folk biological classification that have been studied in detail, a large number of highly salient taxa have been discovered which investigators think form the core of any folk biological taxonomy. Bulmer refers to such taxa, in his Kalam zoological research, as "speciemes" in that they constitute the smallest biological discontinuities in nature which are readily perceived on the basis of numerous characters of form and behavior (Bulmer 1968, 1970; Bulmer and Tyler 1968). Conklin's early study of Hanunóo ethnobotany refers to these fundamental taxa as "basic plant categories" (1954:163). My colleagues and I, following what appears to be a long historical precedent, have chosen to refer to these basic classes as *generic taxa*, as did Bartlett (1940)—in his early description of the generic concept in botany—and Wyman and Harris (1948) and Wyman and Bailey (1968) in their characterization of Navajo ethnobotany and ethnoentomology. The appellation "generic" for this fundamental set is used by Hays (1974), Brunel (1974), Hunn (1973), and Turner (1973) in their recent full-scale ethnobiological descriptions. While the precise label eventually agreed upon by folk biologists to refer to these elementary taxa is, of course, of little consequence to the real issue at hand—understanding the bases of folk biological classification—it may be important to explain why this particular designation has been selected for the present formulation since it has not yet been discussed in print.

Historical Antecedents

The word "genus" and its possible derivations, with essentially the same meaning as intended here, has had a long history in Western biological classification. This should not be surprising, for as Bartlett has pointed out in reference to the study of the plant world: "Classical botany was folk science" (1940:350).

Cain, in his depiction of the place of the genus in early evolutionary theory, is explicit in noting that the genus was thought to have the same essential characteristics as the folk generic classes discussed here. Thus,

"the *idea* of the genus" is formed to deal with the "smallest 'kind' of plant or animal *that can be recognized without close study*"(Cain 1956:97; italics mine. It will be important to recall this passage in the discussion of folk *species* which follows later).

Greene's early, excellent study of the development of botanical thought shows clearly that the genus, as groups of the smallest, easily recognized "kinds" of organisms, formed the basis of Theophrastus' botanical classification (Greene 1909), and Cain (1958, 1959) has the same view of the writings of Linnaeus. Thus, an essential feature for Linnaean classification

> was that its primary category, as being most stable and memorable of all, was the *genus not the species.* All botanists (and by implication all zoologists and mineralogists) must know their genera, which must be separate, distinct entities with distinct names, and natural so that all would agree on their limits. . . . The genus was the *most useful practical unit* (1958:235, italics mine).

While Darwin's revolutionary ideas on evolution were supposed to have radically changed the foundations of biological taxonomy, in actual practice orthodox views on the fundamental nature of the genus continue to the present day. Thus, Simpson is required to write:

> It frequently appears that the genus is a more usable and reliable unit for classification than the species. In dealing with classifications not erected or revised by modern and evolutionary standards—and many such must still be dealt with [*sic*]—it is often questionable whether its 'species' are such by our defintions and not morphs or typological varieties, mutants, ecotypes, demes, subspecies, or something else. 'Genera' are more likely to be acceptable taxa by modern definition, whether we would not assign them specific, generic, or some other rank. Even in modern usage, genera are often more clearly definable and defined than either infra- or suprageneric taxa. They are likely to lend themselves to more clear-cut characters-in-common diagnosis, lower categories, even species, being less clearly distinct and higher categories have few diagnostic characters in common and sometimes none (1961:199).

Terminologically, it may be considered unwise to utilize a term such as *genus* (and later on, *species* and *varietal*) for folk taxonomic notions when these terms now have rather precise—and distinct—meanings in modern biosystematics. Already some confusion has arisen on this very point (cf. Bulmer's 1974 critique of Berlin, Breedlove, and Raven). The competing terms ("basic plant/animal type," "fundamental category," or "specieme") are, however, somewhat ambiguous or jargonistic. Nonetheless, I have no particular investment in using one set of terms or another since, as I have said above, the important issues have to do with the nature of the taxa themselves, not what we may choose to call them.

Distinguishing Characteristics of Generic Taxa

While there may be general agreement among folk biologists as to the significance of generic taxa, there is little agreement concerning the criteria to be utilized in assigning some class of plants or animals to generic rank. Although there is no definitive set of criteria which will be unanimously accepted at this time, it now appears that a combination of taxonomic, linguistic, psychological, and biological features are always

involved in making decisions of generic rank assignment in the actual field situation. I believe that a balanced consideration of the criteria outlined below will make possible the determination of the greater majority of all generic plant and animal taxa in any folk taxonomy. I do not claim that these criteria necessarily permit the formulation of a foolproof set of discovery procedures which can be automatically employed by someone with no experience (or interest) in the substance of folk biological description. Matters of judgment are always involved, as they are in all but the most sterile areas of scientific inquiry.

(1) Generic taxa are consistently labeled in folk biological taxonomies (cf. Bulmer and Tyler 1968; Bulmer 1974 for a contrary view). The nomenclatural properties of generic taxa constitute one of the first and most important features for their recognition in folk biological research, leading Bartlett to provide an essentially linguistic (though logically circular) definition of these groupings. Thus, "the genus . . . is more or less consciously thought of as the smallest grouping requiring a *distinctive* name" (1940:356). Furthermore, generic taxa are given names of a specifiable linguistic structure. Like life form taxa, generic taxa are usually labeled by *primary lexemes.*[3]

In Aguaruna, some 566 generic taxa have been isolated, all of which are labeled by primary lexemes. There is a strong tendency for simple primaries to predominate, as can be seen in Table 1.

(2) Most generic taxa are taxonomically included in one of the few major life form taxa. In Aguaruna, 468 generic taxa, 86 percent of the total inventory, are members of one of the four life form taxa. Their distribution across these major classes is shown in Table 2.

There is a "residue" of ninety-eight generic taxa which are excluded from major life form categories and are, therefore, unaffiliated, or are ambiguously affiliated with one or more life form classes. An *unaffiliated generic* encompasses organisms which, in all contexts of actual plant identification, are consistently said not to be included in one of the major life forms. Thus far, forty-nine taxa of this kind have been isolated.

An *ambiguously affiliated generic* is one which encompasses a group of organisms, most of which are highly polymorphic usually in stem habit. In some contexts of identification, a specimen which is said to be a member of a particular generic may be classified as a member of one life form; in others, a different specimen of the same generic class may be regarded as a member of another life form, or placed in no life form at all. There are at least forty-nine or fifty such taxa in Aguaruna plant classification.

It is informative to examine some examples of the unaffiliated or ambiguously affiliated generic taxa, for it can be shown that, as is the case with the Tzeltal Mayan ethnobotanical materials, such plant classes are "almost without exception cultivated and/or morphologically peculiar in some fashion" (Berlin, Breedlove, and Raven 1973:219).

Of the forty-nine unaffiliated genera, fifteen represent cultivated plants (e.g., *ahéŋ* 'ginger,' *máma* 'manioc,' *dúse* 'peanut,' *idáuk* 'sweet potato,' *kéŋke* 'yam,' *šáa* 'corn'). The remaining thirty-four classes include morphologically distinctive classes of plants, e.g., *čayú dúka,* a twining vine-like

TABLE 1. Examples of Aguaruna Generic Plant Names Exhibiting Three Types of Primary Lexemes

Simple (502 names)
ipák 'achiote' *(Bixa orellana)*
bákaiŋ [no common name] *(Hura crepitans)*
áču 'maurita plam' *(Mauritia peruviana)*
čiŋkán 'bamboo' (*Bambusa* sp.)
datém 'ayahuasca' (*Banisteriopsis* spp.)
híma 'chili pepper' (*Capsicuum* spp.)
čípa [new genus of tree in the Burseraceae]
kái 'avocado' (*Persea* spp.)
dáɛaɛampa [no common name] (*Pothomorphe* spp.)

Unproductive complex (48 names)
iwánči papaihí [no common name] *(Carica microcarpa)* lit. 'devil's papaya' < *íwanč* 'devil,' *papai* 'papaya'
hémpe umpuáŋbau [no common name] (*Clidemia sprucei, Maieta guianensis, Tococa* spp., all small herbaceous spp. in the Melastomataceae) lit. 'hummingbird's manioc garden < *hémpe* 'hummingbird,' *umpuáŋbau* 'manioc garden'

Productive complex (16 names)
činčíp dáek (unidentified vine in the Araceae) < *činčíp* (unanalyzable constituent) + *dáek* 'vine'
númi ménte (unidentified tree in the Bombacaceae) < *numí* 'tree,' *ménte* (unanalyzable constituent)
takašú dupáhi (several unidentified herbs in the Acanthaceae, Rubiaceae, and Gesneriaceae) *takáš* 'toad' + *dúpa* 'herbaceous plant'

Number of generic names = 566

palm; *ikamáš*, a cactus (the only such form collected in the area); *kahijís, čiŋkán, taŋkán*, all types of bamboo; *winčú, tumpéa, jaamáš*, all distinctive species of the broad-leafed banana-like genus *Heliconia; kuíš, wasákea*, the former referring to most epiphytic bromeliads, the latter to a remarkable ground variety.

Of the ambiguously affiliated generics, the most interesting refer to groupings of plants which are morphologically diverse. The generic *činčák* is a good example. This taxon covers a major portion of those

TABLE 2. Distribution of Aguaruna Generic Plant Taxa in Terms of Life Form Affiliation

númi	'tree'	275
daék	'vine'	93
dúpa	'herbaceous plant'	70
šíŋki	'palm'	30
		n = 566

species of plants in the region which are botanically members of the Melastomataceae, a common family of tropical plants. Members of this family are unambiguously recognized by a unique leaf venation pattern which shows three to nine longitudinal nerves. Species of this family have variant botanical life form characteristics at maturity, and they include trees, shrubs, herbs, and rarely, herbaceous vines. Of the sixty-two collections of melastomaceous plants given the name *činčák* in my data, forty-three are assigned to the life form *numí* 'tree,' fourteen to *dúpa* 'herb,' three to *daék* 'vine,' and two are considered to be unaffiliated.

A second illustrative form with similar botanical diversity is *úntuntup*, an Aguaruna generic that covers a major portion of the ubiquitious family Piperaceae. While some species in the family are recognized with distinct generic names, it appears that the term *úntuntup* is utilized to label a "residual category" (cf. Hunn 1973) of piperaceous plants which are not included in any other name. Like the melastomes, pipers (especially, members of the genus *Piper*) show a wide range of growth forms. Of the fifty-two collections of plants labeled by the term *úntuntup*, twenty-eight are placed in the life form *númi*, fifteen in the life form *dúpa*, and nine are considered to be unaffiliated.

The theoretical significance of such ambiguously affiliated generics is of some importance to Kay's "typicality convention" (Kay 1975) discussed in his recent treatment of folk taxonomic structure. Thus, while in English one can say with some confidence that an *oak* is a *tree* (in that all *typical* oaks are trees), I have not been able to determine if *úntuntup* or *činčák* have some members which are more "typical" than others. Unfortunately, my data are not sufficiently complete at this time to allow me to address this problem in detail, though I hope to do so in the near future.

(3) Since generic taxa mark the smallest classes of plants and animals that do not require much close study to recognize, most generic taxa are, as one might expect, *monotypic*. That is, most are terminal and include no further named subdivisions. Those generic taxa which are polytypic exhibit some special botanical, nomenclatural, and psychological characteristics which mark them as a special set (see section on specific and varietal taxa, below).

Approximately 82 percent of all Aguaruna generic taxa are monotypic, and the remaining 103 taxa (18 percent) are further subdivided into *folk specific classes* comprised of from two to thirty or more members.

The ratio of monotypic to polytypic taxa found for Aguaruna folk generic taxa is curiously close to that discovered in other systems of folk biological classification which have been recently described. Hays (1974) finds that 14 percent of the generic taxa for the Ndambu of New Guinea are polytypic. The number of polytypic generic forms in Tzeltal folk botany is 16 percent (Berlin, Breedlove, and Raven 1974:91) and that for Tzeltal folk zoology is 17 percent polytypic forms (Hunn 1973). Brunel, working in an area considerably less rich botanically, reports that 11 percent of the generic plant taxa for the Chacan Quechua are polytypic. Conklin's Hanunóo data, however, do not support this trend: 43 percent of his generic plant classes are further subdivided. Nonetheless, it is worthwhile considering the possibility that a specifiable constant of polytypy—

somewhere in the range of 15 percent—may be characteristic of many ongoing systems of folk classification and might possibly be explained as a function of a subtle but specifiable interplay of biological and cultural constraints.

The distribution of polytypic genera in Aguaruna seems to follow a regular pattern found in many biological lexicons, a pattern described formally by Geoghegan (1976). The majority of all polytypic genera are bitypic, including only two specific taxa. A smaller number include sets of three taxa, and so on at a rapidly diminishing rate. The actual distribution for Aguaruna is seen in Table 3.

TAXA OF SPECIFIC AND VARIETAL RANK

Subgeneric taxa in folk biological classification can be said to have *specific* or *varietal* rank. Taxa which occur as members of these ranks differ from taxa of higher rank in several respects. Taxonomically, most specific taxa occur in sets of two or three members. It is rare for a set of folk species to exceed six, and such sets are invariably organisms of major cultural importance. Varietal taxa are rare in folk taxonomies generally (cf. Hays 1974). Biologically contrasting specific taxa differ on the basis of very few morphological characters; frequently these are readily visible and sometimes verbalizable. Nomenclaturally, specific and varietal classes are labeled, with predictable exceptions, by binomial *secondary lexemes.*[5]

The data for the 104 polytypic Aguaruna generic taxa conform closely

TABLE 3. Distribution of Polytypic Generic Plant Taxa in Aguaruna

No. Included Folk Specifics	No. of Generics
1 (i.e., monotypic genus)	463
2	68
3	12
4	9
5	2
6	3
7	2
8	2
9	1
10	2
.	
.	
.	
21 (bananas)	1
.	
.	
.	
30+ (manioc)	1
	Total 566

to the foregoing general characterization. More than one-half of the specific taxa occur in sets of two members, as do more than two-thirds of the total numbers of sets of either two or three members. All of the larger sets of specific taxa refer to organisms of major cultural importance: e.g., manioc (thirty folk species), banana (twenty-one folk species), ginger (nine), etc. Varietal taxa are infrequent in Aguaruna and have been found only with two important cultigens, yams and bananas.

While generic and life form names are marked by primary lexemes, names for taxa of specific rank in Aguaruna—with important exceptions to be discussed below—are linguistically binomial in form and are analyzed as secondary lexemes. Examples of several specific contrast sets whose members illustrate lexemes of this type are seen in Table 4.

Botanically, the members of each of these specific contrast sets are quite similar except in a few distinctive characters. The four classes of *ípak* are readily distinguished by the shape of the seed capsule as well as the relative abundance and surface distribution of the flexible spine-like protuberances which cover the surface of all but one folk species. The secana types *(Sicana odorifera)* differ in rind color and size and shape of fruit. The two specific types of *ajáŋke* (*mun* and *učí*) apparently differ on one character—the presence or absence of axillary spines on the young growing stem of the vine. Differences between specific taxa of the same contrast are often verbalizable, as well, and I have observed informants avidly discussing the assignment of some specimen to some specific class with enthusiasm.

A NOMENCLATURAL QUALIFICATION

Although binomial nomenclature is the general rule for taxa of specific rank, a number of taxa which are taxonomically included in generic classes are labeled by primary lexemes and, as a consequence, constitute an exception to the binomiality principle. However, the majority of such

TABLE 4. Binomial Specific Nomenclature in Aguaruna Folk Botany

Generic Name	Specific Names	
ipák 'achiote' *(Bixa orellana)*	*baéŋ ipák*	'kidney-achiote'
	čamíŋ ipák	'yellow achiote'
	hémpe ipák	'hummingbird achiote'
	šíŋ ipák	'genuine achiote'
namúk (Sicana odorifera)	*mún namúk*	'large secana'
	eéneak namúk	'dart-like secana'
	ikám namúk	'forest secana'
	kapántu namúk	'red secana'
aháŋke (*Uncaria* spp.)	*mun aháŋke*	'large *Uncaria*' *(U. guianeusis)*
	učí aháŋke	'small *Uncaria*' (unidentified)

monomial specifics in Aguaruna can be shown to be what I have called elsewhere "type specifics." Some of these forms refer to the focus of the category. Reasons for focality include such factors as the cultural importance of the members of the class, widespread distribution, or prominence because of one or more morphological features. On the other hand, monomial specifics may also mark the residue of some folk generics which are not members of any of the contrasting specific taxa in the set. Hunn (1973:120–123) who was the first to my knowledge to note this phenomenon in folk biological classification, calls such taxa "residual categories." Nomenclaturally, all type specific taxa (those that indicate focal members or those that indicate residual categories) are marked with expressions which are polysemous with the label of the superordinate generic. Examples of both types of monomial expressions can be found in Table 5.

Type specific monomials, however, do not exhaust the inventory of monomial specific names in Aguaruna. In several important cultivated

TABLE 5. Examples of Specific Taxa in Aguaruna Folk Botany Labeled by Expressions Polysemous with the Superordinate Generic Name

monomials marking type specifics	*ɛapátaŋ* (*Siolmatra* spp.)	*ɛapátaŋ*	(*Siolmatra mexiae*)
		mun ɛapátaŋ	(no determinations as yet)
	datém (*Banisteropsis* spp.)	*datém*	(*Banisteriopsis* sp.)
		tehés datém	(unidentified)
monomials marking residual category	*saunák* (several species of the Cyclanthaceae)	*saunák*	(residual category of cyclanthaceous plants)
		mun saunák	(exceptionally large cyclanth)
	suŋkíp (several aroid spp.)	*suŋkíp*	(residual category of this group of aroids)
		yawá suŋkíp	both distinctive unidentified species of aroids
		mehén suŋkíp	

plants, specific taxa labeled by primary lexemes have been elicited which cannot be analyzed as examples of type species. This nomenclatural feature is especially common for the critical cultigens banana, manioc, yam, and cocoyam *(Xanthosoma)* (see Table 6).

Data from Terrence Hays on the Ndambu of New Guinea and Nancy Turner's materials from the Pacific Northwest also include cases of monomial specific names which are not analyzable as labels for type species.

TABLE 6. Examples of Specific Taxa in Aguaruna Which Are Labeled by Primary Lexemes *Not* Polysemous with the Superordinate Generic [Monomials designated by *]

sáŋku 'cocoyam' *Xanthosoma* spp.	
(šiŋ) sáŋku	**mančúp*
yamá saŋku	**nehém*
šuin saŋku	**pituk*
páantam ~ *pámpa* 'banana' *Musa* spp.	
**setaš*	**sétu*
**periya*	**tauč*
**katúču*	*kistían pámpa*
**apášmeš*	**kučik čína*
(šiŋ) páantam	**sáŋkusuk*
wakác pámpa	**upíp*
muntuk pámpa	**káki*
**muhát*	**kuŋkuí meheš*
kukúš 'cocona' *Solanum* spp.	
sáwi kukúš	*mun kukúš*
nantú kukúš	**beeáŋ*
**šiwankúš*	**yumís*
kistián kukúš	
máma 'yuca' *(Manihot esculenta)* (30 ∓ well known varieties)	
**puyám*	**hihuántaŋ*
**suhíknum*	**apán*
**kanús*	**uhákaŋ*
ipák máma	**maŋkám*
yakía máma	*yusaŋía máma*
**ukayín*	**ušu uwakín*
**suhiktak*	**dapím*
**tunáim*	**patáku*
**čikím*	**eenkém*
**šámpiŋ*	**wiŋkániŋ*
**kiŋkis*	**činkás*
**paŋmač*	*nampuíŋ máma*
**páum*	**saké máma*
**šímpu*	**yampíeaŋ*
šímpi máma	**antúk*
	**muntúm*

However, such expressions occur in a predictable fashion, and it now appears that where a generic taxon is further partitioned into specific classes, and one or more of the included species are monomially designated (type specifics excluded), *the monomial(s) will invariably refer to a taxon of major cultural importance.* One will not find, in light of this hypothesis, monomial, non-type-specific names for organisms which lack major cultural significance.

One further characteristic of the nature of subgeneric taxa, which needs to be discussed, concerns their relationship to cultural factors. It will be recalled that one important feature of generic taxa was that they represented discontinuities in the biological world which could be easily recognized "without close study." That is, generic taxa are the smallest discontinuities which stand out as easily perceptual and, hence, potentially nameable chunks. Specific (and varietal) taxa, however, seem to be quite different in this respect. Very briefly, it now seems likely that subgeneric taxa are recognized primarily because of the close attention they receive as a result of their cultural significance. (Cf. Geoghegan 1976, for a mathematically sophisticated treatment of the data which follow and some intriguing historical implications.)

What is the evidence for this claim? One kind of data relevant to the topic can be drawn from the distribution of specific taxa over a scale which rates the cultural significance of the plants to which they refer. A gross scale which my colleagues and I have used in our work among the Tzeltal recognizes four categories of cultural importance in terms of which plant types might be rated (Berlin, Breedlove, Raven 1974:99–100). From greatest to least importance, these categories rank plants into *cultivated forms* (consciously planted), *protected plants* (not planted but not consciously destroyed), *significant plants* (recognized useful product but not systematically protected), and *unimportant plants* (no known cultural utility). A similar scale can be used to trace the distribution of monotypic and polytypic generics in Aguaruna folk botany over these four categories (see Table 7).

Table 7 shows that, while a majority of all Aguaruna *generics* have some cultural significance (sixty-one cultivated, forty-five protected, 268 significant), a full one-third of all known plants (189) are conceptually recognized but lack cultural importance. It would thus be an error to suggest that the Aguaruna classify only those plants in their environment which have some particular cultural utility.

TABLE 7. Aguaruna Polytypic Folk Generic Plant Taxa and Their Relative Cultural Significance

	Cultivated	Protected	Significant	Unimportant	Totals
monotypic	37 (15 introduced)	31	215	177	460
polytypic	24 (5 introduced)	14	53	12	103
totals	61	45	268	189	563*

* Actual total 566; 3 generics not classified for lack of data on cultural importance.

However, Table 7 also shows that 81 percent of all *polytypic* generics (hence specific taxa) fall in one of the three categories of cultural importance; *a mere 11 percent of those generics which have been classified as unimportant are polytypic.*

Furthermore, the tendency for a generic to be further subdivided is apparently a function of its cultural importance. Thus, 40 percent of all cultivated plants include folk species; 31 percent of the protected plants are polytypic; and 19 percent of the significant plants include named subdivisions. Generic plants which are considered to be unimportant culturally, however, are overwhelmingly monotypic—a small handful of twelve generics (of the 189 unimportant plants), or 6 percent, are further subdivided into specific classes (Figure 1).

A possible explanation for this apparent lack of subgeneric taxa in plants with no cultural importance—or, for that matter, for those plants with marginal cultural significance—might be that such folk generic groupings exhibit much less internal polymorphism (internal differentia-

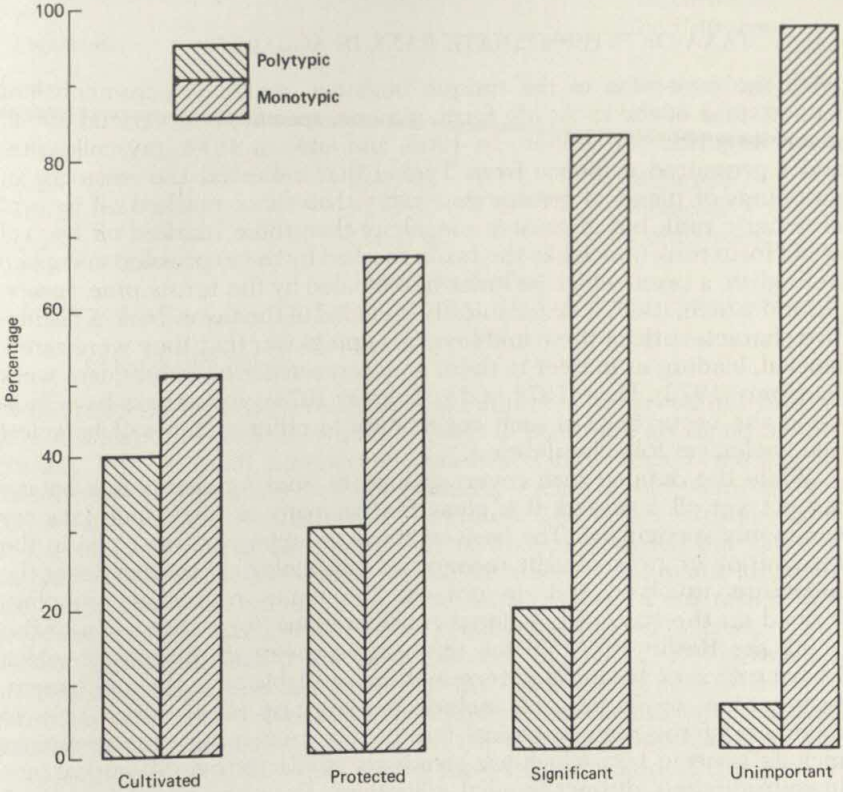

FIGURE 1. Proportion of monotypic/polytypic generic plant taxa by category of cultural significance.

tion) than those with cultural importance. All the evidence available to me now, however, indicates that this is not the case. In fact, the actual numbers of biological species included in generic taxa of little or no cultural importance is many times greater than those included in taxa of major cultural significance. Thus, unimportant generics exhibit more *potential* for further subdivisions than do culturally important forms. I would not discount, however, Bulmer's (1970) and Geoghegan's (1976) persuasive arguments that the objective genetic and morphological characters in cultivated plants caused by controlled breeding must also be considered an important factor in the recognition of subgeneric taxa.

A more likely explanation of these data from Aguaruna (as well as those from Tzeltal and other languages) is that the conceptual recognition of specific taxa is not so much a function of the "close scrutiny" given polymorphic groupings of plants as they exist in nature as it is a result of special attention due to their presumed functional (cultural) importance. In rather unprecise terms, the native folk biologist recognizes generic taxa "because they are there"; he recognizes specific (and varietal) taxa "because it is culturally important to do so."

TAXA OF INTERMEDIATE RANK IN AGUARUNA

With the exception of the unique beginner, which is commonly not named, taxa of the ranks life form, generic, specific, and varietal are all given linguistic recognition. In 1968, and later in 1974, my colleagues and I presented evidence from Tzeltal that indicated the existence of groupings of plants of greater generality than those marked off by taxa of generic rank but of greater specificity than those marked off by taxa of life form rank (much like the taxon marked by the expression *evergreen* in English, a taxon which includes taxa labeled by the terms *pine, spruce, fir,* and which, itself, is taxonomically included in the taxon *tree*). A distinctive characteristic of these mid-level groupings was that they were rarely labeled, leading us to refer to them as *covert categories.* Subsequent work by Hunn (1973), Hays (1974, n.d.), Bulmer (1974), and others have indicated the occurrence of such covert taxa in other systems of botanical and zoological folk classification.

While the data on such covert groupings from Aguaruna folk botany are not yet all analyzed, it is clear that as many as forty such taxa are commonly recognized. The basis of these groupings seems to be in the recognition of gross, visually recognized morphological similarities of the organisms involved and do not—in the main—represent groupings formed on the basis of functional considerations (for a discussion of this point, see Berlin 1974). Of the thirty-eight covert groupings for which I have sufficient botanical determinations available to make a judgment, twenty-nine, or 76 percent, include generic taxa all of which refer to members of the same botanical family. The remaining nine groupings include generic taxa which are strikingly similar in overall appearance in spite of their distinct familial affiliations. Examples of each of these types of covert groupings appear in Table 8.

TABLE 8. Examples of Covert Intermediate Taxa in Aguaruna Folk Botany

Covert Intermediate Taxa	Botanical Species	Family
inák	*Gustavia* sp. nov. 1	Lecthidaceae
inakuám	*Gustavia* sp. nov. 2	Lecthidaceae
tínči *kaíkua* *mantaŋá* *awán*		[all undetermined species in the Lauraceae]
kánea	*Alchornea chordata*	Euphorbiaceae
dátaš	*A. glandulosa*	Euphorbiaceae
pantuí	*Protium* sp.	Burseraceae
čípa	[new genus of tree]	Burseraceae
sukúŋ *náha* *kuyúhimas*		[all nettles in the Urticaceae]
eéke *satík* *súu*		[all members of the genus *Cecropia* in the Moraceae]
yapít	*Ficus* sp.	Moraceae
kásua	*Ficus* sp.	Moraceae
úwe	*Clusia* sp.	Guttiferaceae
	["Strangler figs"]	

CONCLUSIONS

In this report, I have presented the first outlines of Aguaruna folk botanical classification. I have shown that classes of plants are arranged hierarchically into a small number of botanical ranks. Taxa occurring as members of a particular rank are mutually exclusive and share several formal and substantive features in common. The most inclusive taxon at the rank of *unique beginner* is not named consistently, but there is ample evidence that the readily visible members of the plant kingdom are conceptually united into a single semantic domain. Four taxa of *life form rank* have been described which roughly correspond to 'tree,' 'vine,' 'herbaceous plant,' and 'palm.' Some 566 taxa of *generic rank* have been attested thus far. These taxa represent the fundamental core of the folk taxonomy and can be recognized by linguistic, taxonomic, botanical, and psychological criteria. Some generic taxa are further subdivided into taxa of *specific rank*, and 296 such classes have been attested at this time. I have suggested that Aguaruna recognize specific taxa primarily because of cultural considerations which bear on the functional utility of the organ-

isms to man. *Varietal taxa* are rare in Aguaruna and have been found for only two important cultivated plants. Finally, several well-defined *intermediate* groupings of conceptually related generic taxa have been isolated which conform closely to scientifically recognized families or, when crossing family boundaries, are nonetheless homogeneous in the gross morphology of their members. These taxa are *covert* and are not linguistically recognized.

Problems remain in determining the best treatment of some taxa, especially the ambiguously affiliated generic forms, and I have shown that nomenclatural characteristics of certain taxa of specific rank are less systematic than suggested in 1973. Nonetheless, the vast majority of conceptually recognized plant classes in Aguaruna are easily accommodated into one of the proposed ranks in a natural and straightforward fashion. These data suggest that ranks are neither arbitrary nor a mere typological cataloging device invented for the convenience of the ethnographer. On the contrary, the Aguaruna's view of the plant world provides additional support for the hypothesis that the concept of rank is fundamental to all systems of folk biological classification.[6]

NOTES

1. The Aguaruna represent approximately 20,000 manioc cultivating aboriginal Americans who reside in widely dispersed hamlets and villages along the Upper Marañón River and west of the Santiago River in the Department of Amazonas, Peru. Their major area is just to the east of the true Amazon Basin in what is often referred to in Peru as the *montaña*.

   The elevations inhabited by these people range from 200 to 1000 meters above sea level; their total extension is more than 30,000 sq. km. (Guallart 1964). The region is covered by tropical rain forest, and travel off the major rivers and tributaries is difficult and, during a goodly portion of the year, impossible. The area exhibits considerable biological diversity, the rough topography of broken hills and creeks contributing to a high degree of biological endemism. In comparison with many areas of the American tropics, this portion of Peru is very poorly known botanically and zoologically.

   Linguistically and culturally, the Aguaruna are the largest of four tribes which make up the so-called Jívaroan language family (McQuown 1955:512; Siverts 1972; Varese, et al. 1970). Of the remaining three groups—Huambisa, Achuara, and Jívaro proper—only the latter has been given full ethnographic treatment (Harner 1963, 1970, 1972).
2. All Aguaruna names are given in a standard phonemic orthography with the exception that /ɨ/ is rendered as *e* for orthographic convenience, following Larson (1966).
3. Primary lexemes are either linguistically *simple*, such as *oak, pine, maple,* or *spruce,* or linguistically *complex*. At least two types of linguistically complex lexemes have been recognized. One type includes constituents none of which mark a category superordinate to the form in question, e.g., poison oak, hens-and-chickens, kiss-me-over-the-garden gate, baby's tears, etc. Such expressions can be called *unproductive* (complex) primary lexemes. A second type of complex lexeme includes expressions in which one of the constituents marks a category superordinate to the form in question but which nonetheless contrast directly (occurs in the same contrast set) with simple or unproductive complex lexemes, e.g., tulip tree (which contrasts with oak, maple, etc.), puncture vine

(which contrasts with ivy, passion flower, etc.), or creosote bush (which contrasts with rock apple, broom, etc.). Such expressions can be called *productive* (complex) primary lexemes. Some productive primaries may be abbreviated (e.g., pine tree → pine); others may not (e.g., tulip tree → *tulip).

4. Palm determinations are by José María Guallart (1968:230–251).
5. Secondary lexemes are linguistically analyzable expressions which (1) include one constituent that labels an immediately superordinate taxon and (2) occur in contrast sets whose members are also labeled by secondary lexemes which include an identical superordinate constituent.
6. This report is an expansion of an orally presented paper, "General Implications of Aguaruna Jívaro Phytosystematics for General Principles of Folk Biological Classification" given during the symposium, Folk Systems of Biological Classification, at the 72nd Annual Meeting of the American Anthropological Association, New Orleans, 1973. I am grateful to the participants in that symposium for their critical comments, especially those of Paul Kay, Eugene Hunn, William H. Geoghegan, Terrence Hays, and Robert Randall. In addition, it has been rewarding to discuss issues raised in this paper with Elois Ann Berlin, James Shilts Boster, Chad K. McDaniel, and Craig Molgaard. Finally, I would like to express my appreciation to William H. Geoghegan, Allen Sonafrank, and Ruth Deuel for their computer programming assistance. This research has been generously supported by a grant from the National Institute of Mental Health (MH22012), a University of California Faculty Research grant, and the Language Behavior Research Laboratory as authorized under grant MH25703; it is based on sixteen months of field research during 1972–1973 and the summer of 1974. Botanical collections have been deposited at the Missouri Botanical Garden, and determinations have been made available by Drs. Peter H. Raven, Thomas B. Croat, and the staff of the Garden herbarium.

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5

Talking About Doing: Lexicon and Event[1]

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Sociolinguists have long recognized that full understanding of an utterance must include understanding of the event of which the utterance is a part. The speaker's conceptualization of the event, as indicated in his own terminology, however, is seldom taken into account.

Some studies rely on observation and refer properties of events to sociological concepts, such as gatherings, roles and the like (see, for example, Goffman 1971, reviewed by Fought 1972). Other studies use utterance transcripts as their data base, and account for properties of the transcripts in terms of concepts and structures inferred or postulated as underlying them (see, for example, Gumperz 1972). In neither case is the terminology of the participant's themselves consulted.

One might expect such work within the tradition of cognitive anthropology, or ethnographic semantics, which has concentrated on analysis of folk taxonomies and native categories, but analysis of events is rare. Hymes (1967, 1972) has proposed an etic guide to the structure of speech events, in an approach within which participants' taxonomies or categories have a place, but the application to early data from the Abipon by Sherzer (1970) did not have access to much in the way of native terms. Abrahams and Bauman (1970) have dealt with folk taxonomy for types of event and scene, but Frake (1964*b*, 1972) and Metzger & Williams (1963) remain the only examples of detailed study of the internal structure of events.

Most attention in ethnographic semantics has centered on specific domains (persons, plans, diseases, animals, etc.), especially in so far as they are organized by the hierarchical relationship of inclusion. There have been many reasons for such a focus of attention. Much work in cognitive anthropology has been concerned with domains of cultural knowledge in which such a taxonomic structure appears to be common (cf. Sturtevant 1964, Hammel 1965, Tyler 1969, Conklin 1972). Further, such taxonomies have well understood structural properties that lend themselves to analysis and empirical test.

In analyzing the terminological structure of events inclusion is not as widely applicable. In particular, in a study of the lexicon of urban American heroin addicts, hierarchical inclusion has been found to organize directly only a few items. Part of the addict lexicon, listed alphabeti-

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TABLE 1. Segment of Addict Lexicon

Belt	Ordinary belt, used as a tie (q.v.)
Boot	Repeatedly inject blood–heroin mixture in and out of vein
Bulb	Nipple of baby pacifier used as eyedropper bulb
Collar	Thread or piece of paper used to securely fasten needle to eyedropper
Cook	Heat heroin–water mixture to dissolve heroin
Cooker	Receptacle, such as bottle cap, used to cook (q.v.) heroin
Crib	Residence
Draw	Bring the cooked (q.v.) heroin into the eyedropper
Dropper	Eyedropper
Filter	Material used to strain impurities from dissolved heroin
Fix	A prepared, injectable amount of heroin
Flag	Blood that backs into the works (q.v.)
Get-off	Inject heroin
Gravy	Blood–heroin mixture
High	Pleasurable state, chemically induced
Hit	Enter a vein with the needle
Junkie	Narcotics addict
Match	Ordinary match, used to heat cooker
Nod	Semi- or unconscious state induced by heroin
Nylon	Ordinary nylon, used as a tie (q.v.)
O.D.	Overdose
Rope	Vein
Rush	Sensation of heroin entering system
Shoot	Inject the heroin
Shooting gallery	Place where heroin is injected
Sick	Withdrawal syndrome
Spike	Hypodermic needle
Straight	not sick (q.v.)
Streets	Outside
String	Ordinary string, used as a tie (q.v.)
Stuff	Heroin
Tie	Material used to force veins to protrude for injection
Works	Implements used for injecting heroin

Note: Synonyms have been eliminated for brevity.

cally with glosses, is shown in Table 1. Its elements are diverse—verbs as well as nouns, and names from seemingly different domains. Table 1 does exhibit a lexical set, in that all the entries refer to a single, and central event of the heroin addict's day-to-day life, *getting-off* (injecting the drug). And bases of organization of the lexemes as a set can be found.

ANALYSIS

Inclusion does link together some concepts. *Belt* and *string*, for example, both nouns, are both a "kind of" *tie*. On the whole, however, the relation

Level I:			*Get-off* Process			
Level II:	*Cook* Stage I	*Draw* Stage II	*Tie* Stage III	*Hit* Stage IV	*Shoot* Stage V	*Boot* Stage VI

FIGURE 1. Encoded stage-process distinctions in getting-off.

of inclusion organizes only a small part of the set. If other relationships noted in cognitive anthropology, such as that of part to whole, are added, there is still little gain. The *spike* is part of the *works*, for example, but the relationship, like that of inclusion, is limited in application.

The problem, then, is to find relationships that do apply to the set as a whole. One strategy is to attend, not to nouns, but to verbs. These can be extracted as a first step (see Figure 1). A pattern does emerge. *Get-off*, labels the entire event. *Cook* labels a stage in the process of *getting-off*. In fact, the verbs indicate a two-level structure, such that *get-off* provides a cover term for the process as a whole, and the rest of the verbs label stages in it. The key to the central terminological domain of the heroin addict is a relationship, not of hierarchial inclusion, but of processual stage. This does not complete the structure of the set, but it provides a key.

Note that the first stage in Figure 1 is *cooking*, or heating the heroin and water mixture to dissolve the powder. The second is *drawing*, or transferring the mixture into the *works*. The third, *tying*, involves the application of a tourniquet to facilitate injection. The fourth, *hitting*, involves getting the *works* into a vein. *Shooting*, the fifth stage, refers to the transfer of the heroin from the *works* into the addict's system. Finally, *booting*, which optionally substitutes for *shooting*, is the injection of only a portion of the dissolved heroin. Pressure on the *bulb* is then released, and a blood–heroin mixture is drawn back into the *works*. The process can be repeated several times.

The stage-process relationship, expressed in verbs, gives basic organization to the lexical set, but a number of items remain. Some of these are linked to *getting-off*. The *works* is the instrument by which the process is accomplished.[2] Alternative locations where the event can occur are encoded, such as the *shooting gallery*, the *crib*, or the *streets*. Finally, possible results of the process are also encoded, ranging from the *O.D.* to being *sick*.

The semantic relationships that have suggested this patterning are found in recent linguistic theory (Fillmore 1968). Just as recent work in cognitive anthropology argues that the event is central to the understanding of behavior (Frake 1964*b*, 1969; Hymes 1964; Agar 1973*a*, *b*),

[2] Thus, *works* is both a location from which to *draw* as well as an instrument with which to *hit* and *shoot* or *boot*. Such complex relationships between overt forms and underlying concepts have long been recognized, and it should not be a surprise to find them here.

Get-off	*Junkie* (agent)	*Stuff* (object)	*Works* (instrument)	*Shooting Gallery* *Crib* *Streets* (location)	*O.D.* *Nod* *High* *Straight* *Sick* (result)

FIGURE 2. Getting-off and related lexical items at level I.

so case grammar argues that the verb is central in understanding the semantic-syntactic structure of a sentence. Moreover, the verb carries a "case frame" which specifies the optional and obligatory noun slots that are filled in sentences containing the verb, and the type of relationship that the nouns have to the verb. Figure 2 and its analysis borrow directly from case grammar discussions of noun-verb relationships (agent, object, instrument, location and result).[3] (Note that the dramatistic pentad of Burke (1945) would also provide a set of terms within which items of Figure 2 could be placed (agent, agency, scene, act, purpose (or outcome).)

These case-grammar relationships are useful for finding the relationship of other items of the set. At the second level, where stages in the process are encoded by verbs, we find that other lexical items cluster around specific verbs, in terms of object, instrument, result and location, as illustrated in Figure 3. For example, a *filter* is related to *drawing*, since it is used to filter out the undissolved impurities in the cooked mixture when transferred into the works, as instrument. A *tie* is the object used as a tourniquet in the process of *tying*. A *flag* is related to *hitting*, since it names the blood that backs into the hypodermic when a vein is struck, as a result.

A few lexical items remain. These are related to the nouns listed in Figure 3. *Works* label a complex whole whose parts are the *collar, bulb, dropper* and *spike. Tie* is a cover term for a taxonomy that includes *belt, nylon,* and *string.*

The information represented in Figures 1, 2, and 3 can be summarized in Figure 4. Recapitulating, there are three levels. In the first, the entire event is encoded; in the second, different stages in the process are encoded; and in the third, some of the lexical items at the second level indexed related groups of more specific concepts. The relationships used to organize the set have come from two different sources. Notions of inclusion, part-whole and stage-process are mentioned in the literature of cognitive anthropology. The notions of agent, object, instrument and result are taken from recent work in generative semantics. Together these relationships account for the conceptual structure encoded in a set of addict argot terms.

[3] There are many unresolved issues in case grammar that are ignored here, such as the problem of unambiguously mapping case notions onto specific empirical instances, the upper limits on case linkages, and so on. In spite of such problems, the notions do prove useful.

Cook	*Draw*	*Tie*	*Hit*	*Shoot*	*Boot*
stuff	fix	tie	rope	fix	gravy
(object)	(object)	(object)	(object)	(object)	(object)
cooker	works		flag	rush	rush
(location)	(location)		(result)	(result)	(result)
	filter		works	works	works
	(instrument)		(instrument)	(instrument)	(instrument)

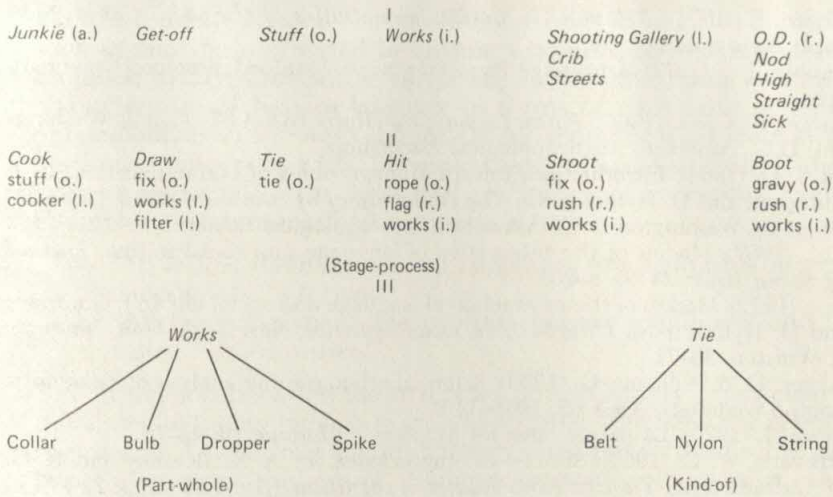
FIGURE 3. Lexical relationships at level II.

DISCUSSION

The structure of Figure 4 is an exemplification of Frake's (1964*a*) goal of describing networks of concepts related in different ways. Certainly the analysis reveals a structure that is a better approximation of a junkie's knowledge than a traditional lexicographic list of items and glosses in alphabetical order. Further, the analysis is not difficult. Perhaps a reason that such analyses are scarce in cognitive anthropology (for a notable exception, see Werner, in press) is simply the nature of the domains and relationships emphasized in previous work. Cognitive anthropologists, interested in domains such as kinship terms, ethnobotany, color, and the like, have emphasized relationships that tie together concepts of the same type, entities with entities, states with states, processes with processes. In terms of linguistic categories, such relationships usually link nouns to nouns or verbs to verbs. Linguistics, on the other hand, interested in sentences, have considered relationships that link concepts marked by different form-classes, notably nouns with verbs. Notions developed in syntax and semantics, then, may fill a gap in the relational repertoire of ethnographic semantics.

There is a feature of polysemy of possible general interest in Figure 4. The label for the event is polysemous with the label for one of the stages in the event. Thus, *get-off, take-off, shoot-up, fix, make-up,* and so on, can label either the entire event or one of its stages. Reasonably enough, the stage is that in which the heroin is actually injected into the vein, the primary focus or climax of the event. Perhaps other studies of events will find that the crucial stage in the process also shows polysemy with the more inclusive event label.

A final implication of this study has to do with the nature of a special lexicon such as the addict argot. Many who discuss argots stress their boundary-marking or secrecy-maintaining function. While boundaries are certainly implied by the use of argot, and while secrecy is maintained by its use (though not from policemen, who quickly learn it), argot does also enable the addict to talk about his experience. Standard English does not of itself provide a standardized way to talk about some of the most important things he must do as a junkie. As he learns the events, he also learns a standard way of talking about them, a way that transcends racial, regional, and age variations. Put more generally, such a special language encodes special events in a way rather analogous to the technical terminology that may be specific to an occupation or profession. This is not to say that the special language does not fill other functions; indeed, language generally fulfills many. It is only to caution against preoccupation with the more exotic aspects of urban deviant groups.



Key: a: agent; o: object; i: instrument; l: location; r: result.

FIGURE 4. The structure of the sample segment of the addict lexicon.

In this connection, notice that some standard English items (*belt, nylon* and *string*) are included as part of the argot, while *match,* though used, is not. This discrepancy occurs only at the lowest level of the taxonomy, which may lend some support to the importance of boundary-maintenance in the jargon. That the discrepancy can be placed may be an indication of the usefulness of the present framework for getting at the articulation between specialized and general terminological systems.

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6

Colour and Kinship: Two Case Studies in "Universal Semantics"

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WHAT DO WE MEAN BY SEMANTIC UNIVERSALS?

One of the recurring speculations of linguistics is: how far is it possiple to apply the same semantic analysis to all natural languages? How far, that is, are the rules and categories of meaning, . . . characteristics of the human faculty of language, wherever it may manifest itself? It is commonly felt that the "deeper" one gets into the substructure of language (i.e., the further one abstracts from the physical substance of language towards its conceptual content) the nearer one gets to a common core of linguistic universals.

But debate about universals can easily become confused unless certain distinctions are made. The first distinction is that made by Chomsky (1965:27–30) between *formal* and *substantive* universals. Formal univer-

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† Source: *Semantics* (Penguin Books, London, 1974), pp. 232–247.

sals are, roughly, general characteristics or rules of language construction such as must be postulated by anyone who aims to construct a general linguistic theory; substantive universals, on the other hand, are universal characteristics of human language in terms of what units or elements or components a language contains. On the semantic level, we may associate formal universals with "universal rules of logical structure" and substantive universals with "universal categories of conceptual content." Examples of statements postulating each type are:

(a) "All lexical definitions in all languages are analysable as a set of components." *(formal)*
(b) "All languages have the contrast between 'animate' and 'inanimate.' " *(substantive)*

The distinction between the two types of universality is not clear in all details, but it is easy enough to see why the first kind need not presuppose the second. Any serious linguistic theory must put forward some general hypotheses about the nature of human language, otherwise it ceases to have any interest except as an *ad hoc* procedure for analysing this language or that. (Thus, by aspiration at least, the bulk of the statements made in the past five chapters of this book have been statements about the universal character of human language: they would fail in generality if it were discovered that they were inapplicable to some languages.) Belief in formal universals, that is, is usually taken for granted by any theoretically inclined linguist.

On the other hand, a linguistic theory can get on quite well without substantive universals, and may in fact deny their existence. One can postulate some general principles of syntactic structure, for instance, and deny that there is some category "noun" which is identical in all languages. Similarly for semantic analysis: one can believe in the applicability of componential analysis to all languages, without insisting that all languages make use of a contrast between +MALE and −MALE. Hence most of the discussion and disagreement on this subject centre round substantive universals.

A second distinction, within the category of substantive universals, should be made between a strong and weak interpretation of what "universal" means. The strong version of a universal hypothesis would say "all languages have a category *x*." But common observation of variation between languages convinces us that in many cases at least, a claim of this strength is false. So with semantic features, as with phonological features, it is natural for a weaker version of a universal hypothesis to be proposed. This claims that "There exists a universal set of semantic features, of which every language possesses a subset." Pressed to its furthest extent, this hypothesis is so weak as to be vacuous: it could be satisfied by the limiting case of a purely "Whorfian" world in which every language possessed its own set of unique features, and in which there was no degree of conceptual identity between languages at all. In practice, such a hypothesis becomes less weak to the extent that we are able to discover that the *same* semantic categories are operating in *different* languages. But the decision to espouse the weak universal hypothesis

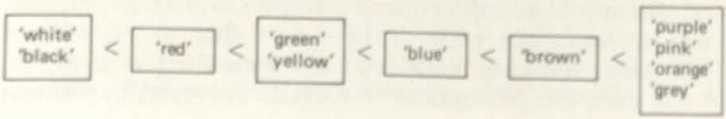
for semantic features and oppositions is a matter of principle rather than substance at the present stage of our knowledge: it means that categories of meaning can be regarded as "language-neutral," i.e. as belonging to the common human faculty of language rather than to the ability to speak this language or that. "Language-neutral hypothesis" might, indeed, be a better term to use for this case than "universal hypothesis." I shall nevertheless adhere to the usual practice of referring to both cases as "universal," distinguishing where necessary between *strong* universality (= "all languages contain *x*") and *weak* universality (= "*x* is a member of a universal set").

One reason for the tentativeness of discussion of semantic universals is a scarcity of detailed research on the comparison of conceptual systems in different languages. But there are two notable exceptions to this generalization. Much attention has been given to the semantic fields of colour and kinship—two fields that have attracted the analyst's attention both because of their intrinsic interest to anthropologists and others, and because of their relative isolability, as conceptual spheres, from the rest of the language. Rather than give any speculations of my own on semantic universals, therefore, I shall give a brief and simplified account of semantics in these two areas, and from this draw what conclusions I can about the plausibility of a "universalist" view of meaning.

COLOUR TERMINOLOGY: THE HYPOTHESIS OF BERLIN AND KAY

In the field of colour terminology, the study by Berlin and Kay *Basic Colour Terms*. . . is based on a comparison of almost a hundred languages. Berlin and Kay's book is remarkable not only for its coverage of data from a wide range of diverse languages, but also for the surprising support it gives to universalism on a terrain previously regarded as a happy hunting-ground for relativist semantics. It has, in the past, seemed almost too easy to show that the systems of colour terminology of different languages differ widely and unpredictably in the way they cut up the "continuum of colour." Contrasting colour-charts for such languages as Hanunóo and English . . . show this clearly in diagrammatic form. Yet from the unpromising diversity of material from every major part of the world, Berlin and Kay arrive at the bold hypothesis that there is a universal set of exactly eleven colour categories, from which each language takes a subset.

The claim of Berlin and Kay is an unusually precise one: not only do they say there are eleven basic categories ('white,' 'black,' 'red,' 'green,' 'yellow,' 'blue,' 'brown,' 'purple,' 'pink,' 'orange,' and 'grey'), but that these categories are ordered (or in strict mathematical terms, partially ordered) as shown:



The ordering relation indicated by the symbol < represents "conditional universality," and is explained as follows: for any two colour categories [*x*] and [*y*], [*x*] < [*y*] means that if a language contains *y*, it must also contain *x*. On this basis, it is possible to set up a small number of major types of colour vocabulary (Type 8 in the table is a category subsuming Berlin and Kay's Types 8–22):

Type	*No. of terms*	*List of Terms*	*Example of language*
1	two	'white', 'black'	Jalé (language of the New Guinea highlands)
2	three	'white', 'black', 'red'	Tiv (Nigeria)
3	four	'white', 'black', 'red', 'green'	Hanunóo (Philippines)
4	four	'white', 'black', 'red', 'yellow'	Ibo (Nigeria)
5	five	'white', 'black', 'red', 'green', 'yellow'	Tzeltal (Mexico)
6	six	'white', 'black', 'red', 'green', 'yellow', 'blue'	Plains Tamil (India)
7	seven	'white', 'black', 'red', 'green', 'yellow', 'blue', 'brown'	Nez Perce (North American Indian)
8	eight, nine, ten, or eleven	'white', 'black', 'red', 'green', 'yellow', 'blue', 'brown', 'purple' and/or 'pink' and/or 'orange' and/or 'grey'	English

The difference between Types 3 and 4, it will be seen, is not a question of the number of terms, but of whether 'green' or 'yellow' is the fourth term added: there is a possibility that either 'green' occurs without 'yellow' or that 'yellow' occurs without 'green,' so that no ordering relation can be set up between these two categories. The four colours 'purple,' 'pink,' 'orange,' and 'grey' are unordered for the same reason, and for convenience are placed together in the table.

To the hypothesis so far stated Berlin and Kay add a further "evolutionary" hypothesis, which states that the types of vocabulary as ordered above represent a fixed sequence of historical stages through which a language must pass as its basic vocabulary increases. (Types 3 and 4 represent alternative stages; Type 8, on the other hand, can be regarded as representing a single final stage of development, as the last four terms 'purple,' 'pink,' 'orange,' and 'grey' tend to get added quickly and in no fixed order.)

To anyone familiar with the apparent arbitrariness and diversity of colour terminologies, their comparative uniformity according to Berlin and Kay may look too good to be true. But Berlin and Kay make it clear that the neatness of this picture depends upon our acceptance of two important assumptions. The first assumption is that it is reasonable to draw a line between "basic" colour terms and other colour terms of secondary importance. For example, *white, red* and *green* are judged basic colour terms in English on such criteria as:

(1) the fact that their range of reference is not included in that of any other colour term (as *scarlet* and *crimson* refer to types of *red*).
(2) the fact that they are not restricted in reference to a small number of objects (as *blonde* is largely restricted to hair).
(3) the fact that the meaning of the whole word is not predictable from the meaning of its parts (as it is in such cases as *blue-green, bluish, lemon-coloured*).

The second assumption is that since people are able to judge the focus or centre of a colour range more easily and consistently than they can judge its periphery, colour concepts should be identified by the foci rather than the boundaries of their range of reference. Accordingly, in a three-colour system, the terms 'white,' 'black,' and 'red' will obviously spread themselves over a wider range of hues and intensities of colour than they will within an eleven-term system; but because their foci correspond, it will still be possible to identify them as "the same category." One consequence of this interpretation of colour terms is, of course, that many objects that would be labelled by the 'red' term in one language would not be labelled by the 'red' term in another; still less, would the 'red' term in one language be infallibly translated by the 'red' term in another. Nevertheless, the important point is that within this system, colour semantics ceases to be completely arbitrary, and becomes predictable within quite narrow limits. As Berlin and Kay are keen to point out, there are, mathematically speaking, 2,048 possible combinations of eleven categories, whereas only twenty-two types actually occur in their data.

One or two problems arising from the data threaten the orderliness of Berlin and Kay's hypothesis: Russian and Hungarian, for instance, cause embarrassment by seeming to contain twelve basic colour categories. (In Russian, there is no general term for blue, but instead, two terms 'dark blue' and 'light blue'; in Hungarian, a similar situation arises with the category 'red.') But on the whole, Berlin and Kay make a persuasive case for regarding colour categories as "weak" universals; that is, for seeing them not as idiosyncrasies of individual languages, but as part of a general conceptual system common to the human race as a whole.

KINSHIP SEMANTICS: LOUNSBURY AND GOODENOUGH

For those interested in the apparently contrasting semantic systems of different languages and cultures, kinship terminology, even more than colour terminology, has been a centre of interest. The most important contributions in this field have come from scholars with a primary interest in anthropology, such as F. G. Lounsbury and W. H. Goodenough, who were the first to develop the technique of componential analysis . . . to any degree of sophistication. (More recently, Lounsbury has developed a different method of analysis, which we shall not have occasion to discuss in this chapter, involving rules of reduction—see Lounsbury 1964a, 1964b, 1965.)

Like colour terminologies, kinship terminologies have traditionally pro-

vided linguistics with scope for the airing of "relativist" ideas, since the categories of kinship manifestly differ radically from one language or culture to another. But there is also scope for the universalist—we see this from the fact that the "data" for an analysis of kinship terminology is normally presented in terms of a universal or at least language-neutral set of symbols such as F = 'father,' M = 'mother,' B = 'brother,' S = 'sister,' s = 'son,' d = 'daughter,' H = 'husband,' W = 'wife.'

Of course, talking of "universal kinship categories" begs the question of whether there are *cultural* universals of kinship, that is, whether the cultural realities referred to by kinship terms remain constant from culture to culture. Some anthropologists would deny that they do. The situation is clearly different from that of colour terminology—where we assume a common human perceptual reality, shared by all races and cultures. If we propose semantic universals of kinship, we also, by implication, propose cultural universals. This extra assumption must be borne in mind in the discussion which follows.

The technique (see Goodenough 1956, 1965, 1970; Lounsbury 1956, 1964a) starts with identifying the range of reference of a term by a list of "denotata," or specific relationships expressed in terms of the above symbols. Thus the range of the term *uncle* in English can be specified as:

FB ('father's brother'), MB ('mother's brother'), FSH ('father's sister's husband') or MSH ('mother's sister's husband').

The task of analysis is then to set up and justify the significant dimensions of contrast and the components of meaning which distinguish the use of one term from that of another. This entails finding common features in each of the denotata of a term (as 'male,' 'collateral,' and 'one generation above the person from whom the relation is being traced' are features common to the four denotata of *uncle* listed above); so that the disjunctive referential specification of a term (as '*x*' OR '*y*' OR '*z*' . . .) is translated into a componential, *con*junctive listing of features ($a + b + c$).

In English, it is obvious that the dimension of sex (+MALE/−MALE) is important as a distinguishing factor—it is the sole feature that separates 'uncle' from 'aunt,' 'brother' from 'sister,' etc. Another opposition of significance for English kinship usage is the distinction between *lineal* kin (related by vertical descent on the family tree) and *collateral* kin (whose connection involves a horizontal link between two siblings on the family tree). Brothers, aunts, nephews, cousins, etc. are all collateral, while fathers, daughters, grandparents, grandchildren, etc. are all lineal.

As an exemplification of the method, I have chosen part of Lounsbury's analysis of the kinship semantics of an American Indian (Iroquois) tribe, the Seneca (Lounsbury, 1964a). Table I gives a part of the data on which Lounsbury bases his analysis: the data given here covers only consanguineal (blood-related) kin within one generation of ego. (Incidentally, we use the terms *ego* and *alter*, according to anthropological convention, to refer respectively to the person from whom the relationship is traced, and the person who is actually referred to in the kinship term. Thus in the phrase *Charlie's aunt*, Charlie is ego, and his aunt is alter.)

It may reassure the reader later if I warn him now that kinship analyses

TABLE I

1. *haʔnih* 'my father'	F; FB; FMSs, FFBs, FMBs, FFSs, FFFBss, etc.	A
2. *noʔyēh* 'my mother'	M; MS; MMSd; MFBd, MMBd, MFSd, MMSdd, etc.	A
3. *hakhnoʔsēh* 'my uncle'	MB; MMSs, MFBs, MMBs, MFSs, MMMSds, etc.	B
4. *akeːhak* 'my aunt'	FS; FMSd, FFBd, FMBd, FFSd; FFFBsd, etc.	B
5. *hahtsiʔ* 'my elder brother'	B; MSs, FBs; MMSds, FFBss, MFBds, FMSss, MMBds, etc. (when older than ego)	C
6. *heʔkẽːʔ* 'my younger brother'	(same, when younger than ego)	C
7. *ahtsiʔ* 'my elder sister'	S; MSd, FBd; MMSdd, FFBsd, MFBdd, FMSsd, MMBdd, etc. (when older than ego)	C
8. *kheʔkẽːʔ* 'my younger sister'	(same, when younger than ego)	C
9. *akyäːʔseːʔ* 'my cousin'	MBs, FSs; MMSss, FFBds, MFBss, FMSds, MMBss, etc. *also:* MBd, FSd; MMSsd, FFBdd, MFBsd, FMSdd, MMBsd, etc.	D
10. *heːawak* 'my son'	(a) s; Bs; MSss; FBss; MBss; FSss; MMSdss, etc. *for male ego* (b) s; Ss; MSds, FBds, MBds, FSds; MMSdds, etc. *for female ego*	E
11. *kheːawak* 'my daughter'	(a) d; Bd; MSsd, FBsd, MBsd, FSsd; MMSdsd, etc. *for male ego* (b) d; Sd; MSdd, FBdd, MBdd, FSdd; MMSddd, etc. *for female ego*	E
12. *heyẽːwõːtẽʔ* 'my nephew'	Ss; MSds, FBds, MBds, FSds, MMSdds, etc. *for male ego*	F
13. *hehsõʔneh* 'my nephew'	Bs; MSss, FBss, MBss, FSss; MMSdss, etc. *for female ego*	F
14. *kheyẽːwõːtẽʔ* 'my niece'	Sd; MSdd, FBdd, MBdd, FSdd, MMSddd, etc. *for male ego*	F
15. *khehsõʔneh* 'my niece'	Bd; MSsd, FBsd, MBsd, FSsd, MMSdsd, etc. *for female ego*	F

have a mind-teasing quality of mathematical puzzles. The only cure for bafflement is to think hard and hope that the light will dawn!

The glosses 'father,' 'cousin,' etc., are not to be mistaken for English translations of the Seneca words: they are no more than roughly corre-

sponding labels, chosen on the grounds that the *nearest* kinsman denoted by the Seneca term would be denoted by that term in English. Like the labels 'black,' 'white,' etc., for colour categories, they represent merely a useful terminological convention. I shall follow the main steps of Lounsbury's argument, but shall change his presentation (including symbols) where necessary in order to make the analysis easier to follow within the context of this book. The data have already been prejudged by Table I, to the extent that certain groupings, A, B, C, D, E, F have been indicated. The solid horizontal lines, it will be seen, separate different generation groups:

Members of A, B are one generation senior to ego
Members of E, F are one generation junior to ego
Members of C, D are of the same generation as ego.

These features can be symbolized > GENERATION 'senior generation,' < GENERATION 'junior generation,' and = GENERATION 'same generation' respectively. For example: the definition of (1) *haʔnih* 'my father' will contain the feature > GENERATION, while that of (10) *heːawak* 'my son' will contain the feature < GENERATION. The distinctions marked by the dotted lines will be elucidated later: in fact, the main point of the argument that follows is to show what these distinctions are, and why they are crucial to the analysis.

If we take the opposition of "generation" for granted, the next step in the analysis is to take each generation group separately, and to try to discover what other distinctions abstractable from the data are criterial in defining the contrast between one term and another.

For the separation of terms 1–4 so as to assign each a distinctive definition, *two* binary contrasts are all that is required. Yet as Lounsbury points out, at least *three* different possible criteria present themselves:

(1) We can bracket together *haʔnih* 'father' and *hakhnoʔsẽh* 'uncle' in opposition to *noʔyẽh* 'mother' and *akeːhak* 'aunt' on grounds of *sex*.
(2) We can bracket together *haʔnih* 'father' and *akeːhak* 'aunt' in opposition to *noʔyeh* 'mother' and *hakhnoʔsẽh* 'uncle' on grounds of *side* (i.e. relatives on father's side versus relatives on mother's side).
(3) We can bracket together *haʔnih* 'father' and *noʔyẽh* 'mother' in opposition to *hakhnoʔsẽh* 'uncle' and *akeːhak* 'aunt' on the grounds that the sex of alter is the same as the sex of the linking parent of ego in the first two cases, but not in the second two.

As this third criterion is more difficult to recognize than the other two, it is worthwhile repeating some of the data from Table I, italicizing the relevant kin, and showing whether they match or do not match in sex:

1. *haʔnih* 'father'	*F; F B; F* M S *s; F* F B *s;* etc.	match
2. *noʔyẽh* 'mother'	*M; M S; M* M S *d; M* F B *d;* etc.	
3. *hakhnoʔsẽh* 'uncle'	*M B; M* M S *s; M* F B *s;* etc.	do not match
4. *akeːhak* 'aunt'	*F S; F* M S *d; F* F B *d;* etc.	

In the case of the simple father and mother relations F and M, the equivalence-of-sex criterion is satisfied vacuously, by the fact that alter and the linking parent are one and the same person. It can be seen that for the first two terms, the sex of the italicized relations is the same, while for the second pair, it is different.

This last criterion, despite its unobviousness, will be shown to be a more significant one than that of side. But to discover this, we first have to consider in some detail the terms for kin of the first generation junior to ego (terms 10 to 15). One striking observation about this group is that terms are defined differently according to the sex of ego. Therefore, let us initially consider the definitions for a male ego (10a, 11a, 12 and 14) in isolation from the terms for a female ego.

Once again, at least three possibilities arise, and we are faced with the problem of which of these three oppositions is redundant. The three criteria pointed out by Lounsbury are:

(1) Distinction on grounds of sex of alter: 10a and 12 are *male*, while 11a and 14 are *female*.
(2) Distinction on grounds of sex of alter's parent: in 10a and 11a *male*, in 12 and 14 *female*.
(3) Distinction on grounds of equivalence or non-equivalence of sex between alter and alter's linking parent: in 10a and 14 they are equivalent, in 11a and 12 they are not.

To check on distinction (2), we examine the sex of the last symbol but one in each formula; to check distinction (3) we examine the sex of both the last symbol and the last symbol but one of each formula, and see whether they match or not.

These three putative oppositions therefore divide up the four terms under consideration as follows:

(1)

10a 12	11a 14
'male'	'female'

(2)

10a 11a	12 14
'male parent'	'female parent'

(3)

10a 14	11a 12
'same sex'	'different sex'

When we consider the < GENERATION terms for a female ego, the same three possibilities arise, and by inspecting the data and applying these criteria we arrive at the following divisions:

(1)

10b 13	11b 15
'male'	'female'

(2)

13 15	10b 11b
'male parent'	'female parent'

(3)

11b 13	10b 15
'same sex'	'different sex'

However, what is very disappointing about these diagrams is that in two of the three oppositions, the positions of the terms are reversed for the

female ego. If we make use of oppositions (2) or (3), we find ourselves claiming that the term (10) *heːawak* 'my son,' for example, has one meaning when used by a man, and a totally different, contrastive meaning when used by a woman. Clearly it would be a preferable solution, both for the elegance and the economy of the analysis, if oppositions (2) and (3) could be replaced by another opposition which assigned a single definition to the 'son' and 'daughter' terms (10 and 11) irrespective of the sex of ego. Lounsbury therefore discards these oppositions and proposes a further criterion:

(4) Distinction on grounds of sex equivalence or non-equivalence between ego and alter's linking parent.

To check this, in each formula we compare the sex of ego with the sex of the last symbol but one; in the case of the simple son and daughter relationships, equivalence of sex is satisfied vacuously by the fact that ego and the parent of alter are identical. This criterion provides a unitary definition for 10 and 11, and therefore is a more effective substitute for criteria (2) and (3):

(4)
For Male Ego

10a 11a	12 14
'sex equivalent'	'sex non-equivalent'

(4)
For Female Ego

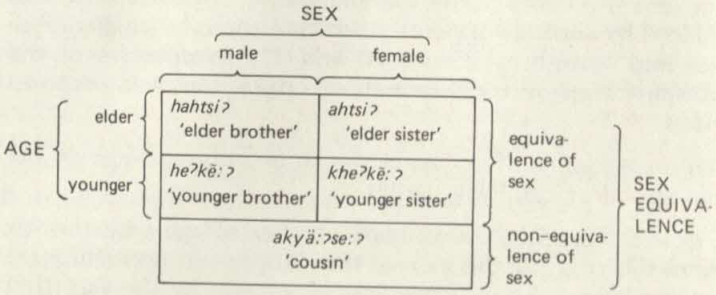
10b 11b	13 15
'sex equivalent'	'sex non-equivalent'

If, as this analysis indicates, the concept of 'son' and 'daughter' rests on sex equivalence between ego and alter's linking parent, it is quite natural that the reference of these terms should vary according to the sex of ego. It is now possible to define the terms of the generation junior to ego:

10. *heːawak 'son'*: < GENERATION + MALE + EQUISEX
11. *kheːawak* 'daughter': < GENERATION − MALE + EQUISEX
12. *heyẽːwõːtẽʔ* } 'nephew': < GENERATION + MALE − EQUISEX
13. *hehsõʔneh* }
14. *kheyẽːwõːtẽʔ* } 'niece': < GENERATION − MALE − EQUISEX
15. *khehsõʔneh* }

The dimension of sex equivalence, here symbolized by the features +EQUISEX/−EQUISEX, evidently has considerable similarity to one of the oppositions considered in the definition of > GENERATION terms. Moreover, when we look at the kin of ego's own generation (=GENERATION), we discover the possibility of a similar contrast there. It can be seen from Table I that the four brother-or-sister terms are distinguished from the 'cousin' term *akyäːʔseːʔ* by *an equivalence of sex between the linking parent of ego and the linking parent of alter.* For example, FBs is classed as a 'brother,' while FSs is classed as a 'cousin.' (In the case of direct siblings, the symbols B and S can be expanded as Fs or Ms, Fd or Md,

in which case the linking parent for both offspring is the same person, and so satisfies the criterion of sex equivalence vacuously.) In diagrammatic terms, the oppositions of these five terms can be represented as follows:



Notice that in the preceding analysis, we came across three contrasts which differ slightly but are all concerned with the equivalence of sex between relatives of the same generation. For senior generation kin, the question to be answered "yes" or "no" was: *Is there a sex equivalence between ego's linking parent and alter?* For junior generation kin, the question was the converse of this: *Is there a sex equivalence between ego and alter's linking parent?* And for kin of the same generation of ego, the question was: *Is there a sex equivalence between ego's linking parent and alter's linking parent?* In the conclusion of his analysis, Lounsbury says that these are all instances of the same semantic contrast, so that the analysis can be simplified by using a single componential opposition (which we may now symbolize ±PARALLEL) instead of all three. He justifies this simplification by reference to a principle less familiar to linguists in semantics than in fields such as phonology and morphology: the principle defined by structural linguists as *complementary distribution*. The essence of this principle is that if two or more linguistic elements are (a) similar in form and (b) non-contrastive with one another in function, then in fact they are not different elements at all, but variant instances of the same element. (On these grounds, for example, the phonologically distinct, though similar, endings of *want*ED (/-id/), *ask*ED (/-t/) and *amaz*ED (/-d/) can be brought together as variants of the same morpheme—the point being that only one of these options can occur in a given context.) In the present instance, the criterion of *similarity* is obviously satisfied, in that all these oppositions involve equivalence and non-equivalence of sex; and the criterion of *non-contrastiveness* is satisfied in that the three oppositions apply respectively to >GENERATION, <GENERATION, and =GENERATION kinship terms. Accordingly we now have a single opposition, ±PARALLEL, which applies in all three generation categories, and which may be specified as follows:

+PARALLEL: 'There is equivalence of sex between the two kin of the generation above ego or alter (whichever of those is junior)'

−PARALLEL: (The negative of the above)

The ±PARALLEL opposition now replaces that of ±EQUISEX used above for junior generation terms. (The choice of label reflects the anthropological distinction between 'parellel cousins' and 'cross cousins.')

Referring back to Table I, we may now see that the broken horizontal lines separating classes A from B, C from D, and E from F, in fact correspond to the +PARALLEL/−PARALLEL distinction. For =GENERATION terms, there is a further opposition of seniority, which may be represented ±SENIOR; and to separate the field of kinship as a whole from other fields of meaning, the feature KIN may be included. With these additions, the componential definitions of terms 1–15 finally run as follows:

1.	*haʔnih* 'my father'	KIN >GENERATION +PARALLEL +MALE
2.	*noʔyẽh* 'my mother'	KIN >GENERATION +PARALLEL −MALE
3.	*hakhnoʔsẽh* 'my uncle'	KIN >GENERATION −PARALLEL +MALE
4.	*akeːhak* 'my aunt'	KIN >GENERATION −PARALLEL −MALE
5.	*hahtsiʔ* 'my elder brother'	KIN =GENERATION +PARALLEL +MALE +SENIOR
6.	*heʔkẽːʔ* 'my younger brother'	KIN =GENERATION +PARALLEL +MALE −SENIOR
7.	*ahtsiʔ* 'my elder sister'	KIN =GENERATION +PARALLEL −MALE +SENIOR
8.	*kheʔkẽː*, 'my younger sister'	KIN =GENERATION +PARALLEL −MALE −SENIOR
9.	*akyäːʔseːʔ* 'my cousin'	KIN =GENERATION −PARALLEL
10.	*heːawak* 'my son'	KIN <GENERATION +PARALLEL +MALE
11.	*kheːawak* 'my daughter'	KIN <GENERATION +PARALLEL −MALE
12.	*heyẽːwõːtẽʔ* 'my nephew'	KIN <GENERATION −PARALLEL +MALE
13.	*hehsõʔnẽh* 'my nephew'	KIN <GENERATION −PARALLEL +MALE
14.	*kheyẽwõːtẽʔ* 'my niece'	KIN <GENERATION −PARALLEL −MALE
15.	*khehsõʔneh* 'my niece'	KIN <GENERATION −PARALLEL −MALE

Further extensions and refinements of this analysis are discussed by Lounsbury; but this brief and simplified sketch has, I hope, illustrated the application of the technique of componential analysis to kinship sufficiently to show how the choice of significant components may be strictly governed by consideration of economy and the maximum generalization of contrasts.

Componential analysis, so considered, is a technique for analysing the kinship semantics of each language in its own terms, without any prior assumptions about a universal set of potential components of kinship. The only prior assumptions are to be found in the data of analysis, the so-called "denotata." The fact that the data for each language can be expressed in terms of the elemental family relationships of the "nuclear family" (F, M, B, S, d, s, H, W) in itself suggests a universal or language-neutral conceptualization of basic kinship relations, even though anthropologists may disagree as to the precise significance of these universal categories, and even though the cultural interpretation of the categories varies from one language to another. Therefore kinship semantics seems to split into two levels—the universal level and the culturally relative level—and componential analysis may be seen as a mediation process between these two.

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* Compiled by R. Casson from the bibliography in Leech's book, *Semantics.*

7

Whorf and His Critics: Linguistic and Nonlinguistic Influences on Color Memory

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INTRODUCTION

Over a period of 25 years, research using color as a stimulus has produced what is perhaps the most cumulative and systematic corpus of basic data on the language and thought question (see Brown and Lenneberg 1954; Burnham and Clark 1955; Lenneberg and Roberts 1956; Lenneberg 1961, 1971; Lantz and Stefflre 1964; Stefflre, Castillo Vales, and Morley 1966; Heider 1972; also see Brown 1976 for a review).[1] During this period, a dramatic reversal of opinion concerning the relationship between language and thought has occurred. As Brown notes (1976:152), color research "began in a spirit of strong relativism and linguistic determinism [influenced primarily by Benjamin Lee Whorf] and has now come to a position of cultural universalism and linguistic insignificance." This interpretive "about-face" represents to Brown (1976:152) the "fascinating irony" of the color-research tradition; to us it seems especially ironic since the evidence we shall present challenges the empirical basis for this reversal of interpretation.

In the early period, from the early 1950s to the mid-1960s, the guiding assumption of the color-research tradition was that the color spectrum was a continuum, and the basic hypothesis tested was that language shaped thought (viz., recognition memory). Brown himself imagined "a universal law relating referent codability to recognition (and perhaps other aspects of cognition) with each different language having its own codability scores for given referents and the speakers of that language having corresponding recognition skills" (1976:129). Stefflre, Castillo Vales, and Morley (1966:112) argued that their subjects' success in delayed recognition of color stimuli is related to communication accuracy," that is, the extent to which "the names or descriptions the subjects give for the stimuli allow other native speakers of the language to pick out the particular stimulus encoded." It was during that early period that Brown (1965:334) characterized the process of delayed recognition as follows:

> When the color initially appears you try to give it a distinctive name. . . . When the color is removed the name can be retained, even rehearsed. Somehow, names are responsive to volition in a way that images are not. . . . When the chip is found which best deserves the name, that is recognition.

The latter period in the color-research tradition extends from the late 1960s to the present. As a result of the important work of Berlin and

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‡ Data appendices in the original article have been omitted in this publication.

Kay (1969), attention shifted to the psychophysical inequalities of the color spectrum. The hypothesis tested (see Heider 1972) was that certain "focal" areas in the color space are salient to the human perceptual system, form the nucleus of basic color terms, and are inherently more memorable regardless of language and culture. In 1972 Heider concluded that memory for color does not vary with changes in linguistic code, and, on the basis of Heider's evidence, Roger Brown (1976; also see 1977) has recently renounced his earlier linguistic hypotheses. Reflecting on the 25-year history of the color-research tradition, Brown (1976:149) remarks: "From the extreme relativism of Whorf and the anthropologists of his day, we have come to an extreme cultural universality and persumptive nativism."

The goals of the present project are to examine critically the empirical foundation of this reversal in opinion and to introduce new evidence regarding the role of language as a factor in human color memory. We assess the relationship between memory and: (1) color stimuli discriminability; (2) focality; (3) two-person communication accuracy; (4) group communication accuracy; and (5) referential confusability. What we discover is that the color array used by Heider (1972) to establish the translinguistic superior memorability of focal versus nonfocal colors is discriminatively biased in favor of focal colors. Under conditions simulating "perfect memory" (that is, in the presence of visible target probes), focal chips are easier to identify in the array than are nonfocal chips. Other results suggest that when Heider's array is modified to make focal and nonfocal chips equally discriminable, various linguistic indicators are better predictors of memory accuracy in both short- and long-term memory than is focality. We demonstrate that, for the most part, whatever influence focality has on memory is mediated by language. We introduce the notion of a "basic" *description* for a color (which is not to be confused with a "basic" color *term*). "Basic" descriptions seem to facilitate memory. Finally, in our discussion section, we revive Whorf's (frequently overlooked) hierarchical interpretation of the relationship between sublingusitic universals (whose existence he never denied) and language (whose relationship to sublingusitic universals Whorf viewed as one of "appropriation," not one of "opposition").

THE COLOR-RESEARCH TRADITION: SOME LOOSE THREADS

In this section, we evaluate two representative studies from the color-research tradition. Our goal is to identify several ambiguous issues that motivate the experimental work to follow.

Despite the shift in interpretive framework, the "color-research tradition" has been unified by a more or less consistent methodology. The basic procedure involves using samples of colors to elicit two sets of responses: one "cognitive," usually recognition memory, and the other "linguistic," usually some measure (typically referred to as codability) of the various properties of expressions that people use to denote the sensation of color. The pattern of association between the two types of responses

is then taken as evidence for or against some specific hypothesis concerning the relationship between language and thought.

As noted earlier, color research can be divided into two periods or, perhaps more accurately, two schools of thought. To characterize better these two schools of thought, the critical experiment from each period will be described and evaluated. The work of Lantz and Stefflre (1964) brought together into a unified framework a series of somewhat discrepant findings from the first period. Except for the replication of their study in a cross-cultural context (Stefflre, Castillo Vales, and Morley 1966), it represents the last empirical effort of the first period. A study by Heider (1972) is regarded by most researchers as the definitive study of the second period; certainly from an experimental point of view, it is the most sophisticated study in establishing the influence of perceptual saliencies in the color spectrum on linguistic forms. Both studies contain careful reviews of the literature and are quite sensitive to the implications of their work for earlier findings. They thus provide a good experimental and theoretical basis for replication and extension.

Lantz's and Stefflre's study actually consists of two parallel studies: one on ease of encoding and the other on similarity of encoding. We will be concerned only with the first of those: ease of encoding, or codability.

After carefully reviewing previous research in which codability is measured by intersubject naming agreement and various indices of brevity, Lantz and Stefflre argue that the contradictory findings in previous research may be caused by the inadequacy of the measure of codability. Therefore, they propose a new measure of codability: *communication accuracy.* They argue that this measure will allow a superior prediction of memory because of a formal similarity between the two activities:

> We will view memory as though it were a situation in which an individual communicates to himself through time using the brain as a channel. This communication process can be approximated by having individuals communicate with other people. Items accurately communicated interpersonally would then be predicted to be more accurately communicated intrapersonally as measured by the usual memory tests. . . . Thus, for our measure of codability, people were presented with a stimulus array and asked to make up messages that would enable another person to pick out the stimulus it refers to . . . [Lantz and Stefflre 1964:473].

In their experimental design, Lantz and Stefflre contrast this new measure of codability, communication accuracy, with the two measures used previously: intersubject naming agreement (Brown and Lenneberg 1954) and brevity (Glanzer and Clark 1963a,b). On the cognitive side, they look at recognition memory using three different recognition conditions: (I) 5-second presentation of 1 chip with a 5-second interval before recognition trial; (II) 5-second presentation of 4 chips with a 5-second interval before recognition trial; and (III) 5-second presentation of 4 chips with a 30-second interval before recognition trial. In addition, they employed both sets of color materials previously used in similar research: the Brown-

Lenneberg array (1954) and the Farnsworth-Munsell array (Burnham and Clark 1955; Lenneberg 1961). Thus, within the experimental design, they are able to assess the importance of stimuli, procedures, and measures. The overall results show that communication accuracy is the most predictive linguistic index for recognition memory and that the results are strongest in the conditions where there are more chips or longer time intervals. Further, certain contradictory findings available in previous research are shown to be caused by the use of different stimulus materials since the Farnsworth-Munsell colors are poorly related to the naming and brevity measures of codability. For purposes of comparison, Lantz's and Stefflre's basic findings are presented in Table I.

Lantz and Stefflre are able to interpret the significant correlations between the naming and brevity measures and the second recognition condition as being caused by the correlation of each of these measures with communication accuracy. Controlling for this relationship reduces the two correlations to near zero (.06 and .07, respectively), leaving communication accuracy as the only significant predictor of recognition memory.

A subsequent project (Stefflre, Castillo Vales, and Morley 1966) investigated the extent to which communication accuracy and recognition memory varied from language to language (and from culture to culture). Using the Farnsworth-Munsell array, Stefflre, Castillo Vales, and Morley assessed communication accuracy in both Mexican Spanish and Yucatec Maya. Recognition memory was assessed using a 5-second exposure of 1 chip with a 30-second delay before recognition testing. (Note that this condition differs from I, II, and III above.) Stefflre, Castillo Vales, and Morley discovered that within each of the two languages, recognition memory was associated with communication accuracy ($r = .44$, $p = < .05$ for Yucatec; $r = .58$, $p = < .01$ for Spanish). While the worst colors (in terms of communication accuracy) were the same for the two languages (implying some cross-linguistic similarity), the best colors differed between the

TABLE I. Correlations Between Codability Measures and Recognition Conditions (from Lantz and Stefflre 1964:477)

	Recognition Condition		
Codability	I	II	III
Farnsworth-Munsell:			
Communication accuracy	.32	.71***	.66**
Naming	−.18	−.05	−.30
Brevity	−.29	.31	.23
Brown-Lenneberg:			
Communication accuracy	.51**	.86***	.78***
Naming	−.02	.40*	.32
Brevity	.19	.42*	.33

* $p < .05$
** $p < .01$
*** $p < .001$

two language groups (implying some degree of cultural or linguistic relativity). Overall, the communication accuracy scores for Yucatec speakers were unrelated to the communication accuracy scores for Spanish speakers ($r = .11$, n.s.). The same was true of the recognition memory scores ($r = .06$, n.s.). In other words, as Stefflre, Castillo Vales, and Morley note, the Spanish and Maya groups "found different colors easy to remember and easy to communicate" (1966:112).

Two types of criticisms have been directed against Lantz's and Stefflre's (1964) and Stefflre's, Castillo Vales's, and Morley's (1966) research on communication accuracy. Brown (1976:145) has argued that communication accuracy is entirely

> a psychological and not a cultural variable. [For a variable to be "cultural," it must be concerned with "shared knowledge and behavior" (Brown 1976:145).] It is directly concerned with individual differences. One person might very well be a highly skilled encoder (or decoder) for a given domain and another very unskilled, though both have the same native language. Communicability scores of individual colors would depend partly on the language, but also on salient objects in the environment.

There are grounds for not being completely satisfied with Lantz's and Stefflre's communication accuracy measure, but to us Brown's particular objection lacks force. Certainly *any* measure (latency of response, brevity of the expressions used to label colors, amount of agreement in the use of referential expressions by different subjects) can potentially be examined from the point of view of individual differences, though that hardly seems like a weakness. Moreover, it is *not* individual differences that Lantz and Stefflre investigated; in fact, they control for individual variation in encoding and decoding. What Lantz and Stefflre and Stefflre, Castillo Vales, and Morley do investigate is the overall (or average) referential accuracy of members of a speech community. Quite properly, their concern is with language use as it might occur in ordinary communicative situations. Thus, their view of language is not restricted to dictionary entries (e.g., "blue") but, rather, includes all the expressions, presuppositions, and background knowledge (e.g., "crayon flesh"; are crayons covered with flesh?!) that make it possible for members of a speech community to understand one another. It is noteworthy that recent work in psycho- and sociolinguistics as well as in the philosophy of language has broken down the distinction between the structural aspects of a linguistic code and the contextual and knowledge factors that support comprehension (Searle 1969; Ziff 1972; Bransford and McCarrell 1974; Goffman 1976).

The communication accuracy measure does suffer, however, in that it does not tell us anything about what aspects of language are important in the communication process. Whereas Lantz and Stefflre do control for individual variation in encoding and decoding, they never extract any picture of the patterned regularities that might be responsible for communicative success. We are left with the black box of "communication accuracy," which may rest upon a variety of factors of the speech situation.

A second objection to the research on communication accuracy con-

cerns the finding that the delayed recognition scores for speakers of Yucatec and Spanish are not correlated. Heider (1972) does not dispute the importance of Stefflre's, Castillo Vales's, and Morley's findings but she does point out that the Farnsworth-Munsell color array used by Stefflre et al. is a low- "saturation" array containing no "focal" colors. She argues that both Yucatec and Spanish speakers would probably have had superior memory for focal versus nonfocal colors, regardless of their linguistic and cultural differences. The reasonableness of that prediction is later examined.

The work of Lantz and Stefflre was not continued by others but was supplanted by a new line of research owing much of its impetus to the publication of Berlin's and Kay's *Basic Color Terms* (1969). Berlin and Kay argue that there is a universal semantic and evolutionary hierarchy for "basic" color terms (e.g., red is basic; maroon is not)[2] such that if a language has N basic color terms, their referential foci can be predicted simply on the basis of that number. Berlin's and Kay's theory spawned a series of studies into individual color lexicons to see whether they fit the hypothesized hierarchy. That line of research eventually came to consider the possible biological or psychophysical basis for the universal hierarchy. Since the findings of that research resulted in claims about the perceptual unevenness of the type of color array used by Brown and Lenneberg (1954) and Lantz and Stefflre (1964), it eventually began to present a serious challenge to the findings of the earlier period of research. The crucial study that attempts to evaluate the role of perceptual saliences in the color arrays in this later period is Heider's (1972).

Heider is specifically concerned with Berlin's and Kay's "focal" colors—colors that are best examples of basic color terms across most languages. Heider argues that "focal" colors are perceptually salient and that this saliency leads in turn to greater memorability (recognition memory) and greater codability (brevity). Since the perceptual salience is presumably universal, so are the focal colors, and so, in turn, are the referential bases for the basic color terms in all languages. Heider conducts four experiments in support of that hypothesis.

In the Western scientific tradition, hue, value (= brightness), and saturation (= purity) are the three psychophysical dimensions necessary to describe completely the color space. In Experiment I, Heider shows that there is a relationship between saturation and basic color terms. She first identifies the hue and value combinations for each of Berlin's and Kay's focal colors. (All colors in their array are at maximum saturation.) She presents subjects with all the levels of saturation available for each such hue-value combination and finds that 90–95% of all choices of the best example of the basic color term associated with that focal are from the two highest levels of saturation. In Experiment II, Heider attempts to show that focal colors are most codable across languages. She finds that for 23 languages (one speaker each) the focals are more codable on several indices of brevity (number of words, number of letters, and response latency) than a selection of nonfocals taken from the array. In Experiment III, focals and nonfocals are contrasted in a short-term recog-

nition memory experiment conducted in two vastly different languages—English and Dani (New Guinea). (Since Heider finds no differences between her two subcategories of nonfocals, we will group them all as "nonfocals" throughout this paper.) Her findings (see Table II) show that the category of focal chips is easier to remember than that of nonfocals. Finally, in Experiment IV, the findings for short-term recognition memory are extended to long-term memory. Heider shows that subjects do better with focals than nonfocals in a paired-associates learning task administered over several days; she argues that superior performance on this task is indicative of better long-term memory. Heider concludes at the end of these four experiments that

> The same colors were most codable in a diverse sample of languages . . . , and those colors were best remembered even in a culture where the linguistic factors of hue terms and codability were absent . . . [Heider 1972:19].

She attributes this superiority to the inherent saliency of the focal colors.

Heider wavers on whether or not her study is one in language and thought. In her introduction, she says she is studying "the relation between a linguistic domain and the nonlinguistic domain which it encodes" (1972:11) which is not "language and thought." Yet, in her abstract she holds that on the basis of her findings "linguistically causal interpretations of earlier language and cognition studies using color were challenged" (1972:10), and in her final conclusion she writes:

> . . . the color space would seem to be a prime example of the influence of underlying perceptual-cognitive factors on the formation and reference of linguistic categories [1972:20].

Given the latter two comments, there can be little doubt that she intends to criticize earlier research. It is noteworthy, therefore, that she does not test directly to see whether memorability and codability are more highly related to each other than would be predicted on the basis of their individual correlations with focality.

Heider does gather extensive cross-linguistic data showing similar memory patterns (better retention of focals) in two societies with different linguistic codes (Dani and English) and similar encodings (in terms of brevity) across a wide spectrum of languages. This suggests that we must take seriously the claim that focality may be a factor independently regu-

TABLE II. Summary of Heider's Findings on Focal/Nonfocal Differences in Recognition Memory (adapted from Heider 1972:16)

		Focal colors (n = 8)	Nonfocal colors (n = 16)
English:	Number correct	5.25 (out of 8)	5.73 (out of 16)
	Correct-response latency	6.0	11.15
Dani:	Number correct	2.05 (out of 8)	1.18 (out of 16)
	Correct-response latency	2.5	3.25

lating codability and memory. Studies from the earlier period do not provide direct information on such a possibility. There is, however, a major weakness in the way Heider makes use of the Dani as a control group.

The Dani have only two "basic" color terms, but, as Berlin and Kay note (1969:5), "every language has an indefinitely large number of expressions that denote the sensation of color." "Basic" color terms (terms such as "white," "red," and "green") comprise but a *very* small subset of the linguistic resources of a speech community for talking about color (see, e.g., Lienhardt 1961). By Berlin's and Kay's criteria, none of the following English expressions would qualify as a "basic" color term: olive, peach, maroon, pea-soup green, salmon, lemon, lilac, lavender, sapphire, emerald, blond, grass green, sky blue, bright navy, pale yellow, aqua, crimson, like an apple, etc.[2] Clearly, the number of basic color terms in a language is not indicative of the ability of a culture to represent a color sensation linguistically. The fact that the Dani have but two "basic" terms tells us little about their ability to generate a linguistic expression for denoting colors. This point takes on special importance when one considers that: (a) the only measure of codability that is known to relate consistently to recognition memory is "communication accuracy" (Lantz and Stefflre 1964; see above); and (b) "communication accuracy" is probably independent of either the brevity of a term or the criteria for identifying an expression for color as a "basic" term (see, e.g., Brown 1965:340). . . . It would seem that Heider's selection of the Dani as a control group was motivated by a commitment to an inappropriate codability measure (that is, *basic* color *terms*).

We can now see that the empirical basis for the shift in opinion outlined in our introduction is somewhat shaky. The most glaring deficit is the absence of any direct contrast of focality and codability as predictors of recognition memory. Lantz and Stefflre do consider the influence of the type of color array but do not examine the competing hypothesis that focal versus nonfocal properties of the color stimuli might explain their results (see Figure 1). Heider shows that focal colors are more codable on some indices and more memorable also, but she neither tests to see whether memorability and codability relate more strongly than their joint correlations with focality would suggest (see Figure 1), nor does she utilize the one linguistic index (communication accuracy) that is known to be a powerful and stable predictor of recognition memory.

The experiments that follow attempt to investigate the interrelations among the triad of variables portrayed in Figure 1. The first three experiments, which retest Heider's findings, involve analyses and modifications of her critical procedure employed with her Dani and American samples. The last two experiments extend and modify Lantz's and Stefflre's approach by investigating communication accuracy in relationship to the focal/nonfocal distinction, by exploring the bases of subjects' performances, and by creating a new, less individual, index of communication accuracy. The entire set of experiments allows an examination of the interrelations among language, thought, and external stimuli.

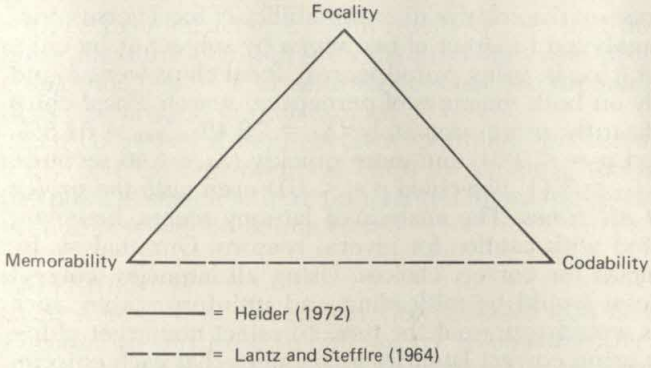

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FIGURE 1. Triad of relations between focality, codability, and memorability, showing the assessments made in previous research.

EXPERIMENTS ON FOCALITY, MEMORABILITY, AND CODABILITY

EXPERIMENT I: DISCRIMINABILITY DURING PERCEPTUAL SEARCH, OR RECOGNITION MEMORY?

Purpose: Heider found a difference in recognition memory between focal and nonfocal colors. Both accuracy and latency scores show this effect. Simple inspection of the array, coupled with an informal attempt to match visible probes with their counterparts in the array, suggested that the focals might have an advantage in both speed and accuracy during the process of perceptually searching through the array, independent of any memory effect. While Heider had computed a discriminability score (1972:17) on the target chips relative to their neighbors in the array to assure that this potential biasing factor was *not* influencing her results, informal inspection seemed to belie that conclusion. Moreover, Heider's discriminability scores were ultimately derived from discriminability tasks administered under experimental conditions unlike her own (see Newhall, Nickerson, and Judd 1943). Hence the present experimental test for a possible biasing relationship between focality and discriminability in Heider's "Dani array."

Method: Eighteen university students (12 women and 6 men) were given a perceptual matching task using Heider's materials. . . . Each subject in turn was presented with each of 25 probes and asked to find the exact same chip in the full 160 color-chip array. . . . Subjects were instructed to match both as quickly and as accurately as they could; both accuracy and latency scores were then recorded. By leaving the probe in full view during the search period, we hoped to stimulate "perfect memory," so that we could assess whether Heider's results might simply come from effects arising during the perceptual search process and not during the memory process.

Results: The evidence on the relative discriminability of focal versus nonfocal chips can be analyzed in either of two ways: by subject or by chip. Analyzed by subject (that is, using paired scores), focal chips were found to differ significantly on both measures of perceptual search. Focal chips were located significantly more accurately ($\bar{X}_f = 78.4\%$, $\bar{X}_{nf} = 64.5\%$, $t_{17} = 3.90$, two-tailed $p = < .001$) and more quickly ($\bar{X}_f = 4.46$ seconds, $\bar{X}_{nf} = 5.34$ seconds, $t_{17} = 3.11$, two-tailed $p = < .01$) *even with the probes visually present at all times.* The analysis of latency scores, however, should be interpreted with caution for several reasons. Our analysis focuses only on latencies for correct choices. Using all latencies (correct and incorrect choices) would be misleading and uninterpretable since many of the figures would represent the time to select nontarget chips. However, although using correct latencies guarantees that each chip receives scores it is entitled to, the possibility remains that the various chips and subjects are unequally represented. Hence, neither measure is satisfactory. Fortunately, the basic measure in most color research, including Heider's, is the accuracy score (number or percent correct); it is this measure that we shall focus upon in our experiments.

Analyzed by chip, the difference in perceptual search accuracy for focal versus nonfocal colors falls just short of significance ($t_{22} = 1.77$ pooled variance analysis, $p = < .10$; $t_{16.7} = 1.79$ separate variance analysis, $p = < .10$). This pattern of findings is summarized in Table III.

Discussion: It seems to us that caution is called for in interpreting Heider's Dani study. The overall pattern of our results suggests that the differences observed by Heider in her short-term recognition memory experiment may be in part because of a difference between focal and nonfocal chips at the point of perceptual search during recall. What this means is that, even if the Dani (or anyone else) had perfect recall, Heider's stimulus array is such that it is more difficult to identify nonfocal than focal target chips. It is important to stress that this perceptual discriminability factor is *not* the same as Heider's "saliency," which is an *intrinsic* property of the color, not a property dependent on neighboring chips. She takes great care to exclude this factor of perceptual discriminability as a possible source of bias in her data since it is known to correlate

TABLE III. Accuracy Scores (Percentage Correct) for Perceptual Search (Discriminability) Task (Experiment I)[a]

$\bar{X}$ focal	78.4
$\bar{X}$ nonfocal	64.5
Three t-test analyses of focal/nonfocal differences	
t_{17} (by subject—18 paired scores)	3.90
p (two-tailed)	<.001
t_{22} (by chip-pooled variance)	1.77
p (two tailed)	<.10
$t_{16.7}$ (by chip-separate variance)	1.79
p (two-tailed)	<.10

[a] Accuracy under conditions simulating perfect memory.

with recognition accuracy. Our results suggest that the use of discriminability measures computed on the psychophysical properties of the color chips (hue, value, and saturation) is not an adequate control and that direct behavioral measures must be taken.

The next experiment involves controlling for the unequal discriminability of focal and nonfocal chips by equating focals and nonfocals at the point of search so that a proper test for an independent effect because of memory may be administered.

Experiment II: Construction of an Array Controlled for Discriminability

Purpose: In this experiment, we try to equalize the error and latency scores for the focal and nonfocal chips while preserving the maximum comparability with Heider's materials.

Method: Equalization of search times and error rates was accomplished by eliminating selected chips from the existing array.[3] The procedure for elimination involved discarding chips that were erroneously selected as targets two or more times in the original search task. Thirty-one chips met that criterion without complications. Two chips remained that also met the criterion but were themselves target chips (two nonfocals) from the original set of 25. Rather than eliminate these two chips, a third target chip (also a nonfocal) that was the *source* of the errors for the other two was deleted; this brought the other two target chips below the criterion. This deletion was a superior procedure because it equalized the number of targets (at eight each) for the focals and Heider's two categories of nonfocals, and it allowed the construction of a rectangular array. Rearrangement of the array was necessary because removal of 32 chips left "holes" in the original array of colors. Hence, the remaining 128 chips (left after eliminations) were randomly reordered into an 8-by-16 grid. This new array had the advantage of randomizing the effect of neighboring chips on identification of targets; the original array had not provided such randomization, and the very obvious "structure" in that array undoubtedly influenced the search times for the various colors.

The procedure of Experiment I was then replicated using this new array with ten subjects. This new array was not perceptually equal either. With the probe in full view, all ten subjects were more accurate in identifying focal chips ($\bar{X}_f = 91.3\%$, $\bar{X}_{nf} = 73.9\%$, $t_{(9,\ \text{by subject})} = 8.83$, $p = < .001$). Latency scores also continued to favor focal chips ($\bar{X}_f = 7.91$ seconds, $\bar{X}_{nf} = 9.96$ seconds, $t_{(9,\ \text{by subject})} = 2.48$, $p = < .05$). Consequently, the above procedure for making the array discriminitively fair was repeated a second time by discarding a further 8 chips, effectively deleting 1 row (substituted into the 8 openings), leaving a final array of 8 by 15. This array was tested on 15 university students (9 men and 6 women).

Results: The array just described showed no significant difference between focals and nonfocals either on percent correct ($\bar{X}_f = 89.4\%$, $\bar{X}_{nf} = 82.2\%$, $t_{(15,\ \text{by subject})} = .34$, n.s.) or on latency for correct choices ($\bar{X}_f = 7.76$ seconds, $\bar{X}_{nf} = 8.44$ seconds, $t_{(15\ \text{paired scores})} = 1.49$, n.s.). Note

that what differences do exist show the focals as faster and more often correct. . . .

Discussion: The results of the experimental test confirm that the final 120-chip array constructed by elimination and randomization showed no statistically significant difference between focal and nonfocal chips in error rate or in latency on correct responses. (It is interesting to note than randomization alone would not have created an unbiased array since the bias persisted after the first rearrangement.) This array, then, presents a fair set of materials with which to test for the hypothesized superior memorability of the focal colors.

EXPERIMENT III:
RECOGNITION MEMORY FOR FOCAL AND NONFOCAL COLORS

Purpose: Experiment III attempts to: (1) replicate Heider's experiment concerning the difference between focal and nonfocal colors in short-term recognition memory by using the new array developed in Experiment II; (2) test for a difference between focals and nonfocals in long-term memory with a more straightforward measure than Heider's paired-associates learning task; and (3) explore through informal observation and questioning the nature of subjects' errors and their subjective understandings of their memory techniques.

Method: There were two experimental conditions for each subject: a short-term one and a long-term one. The basic procedure for both conditions was the same as that employed by Heider: a subject was presented with a chip to view for a set amount of time; then the subject passed a certain length of filler time without the chip in view; and finally, the subject tried to select (recognize) the target out of the full color-chip array when it was uncovered. In the short-term condition, presentation was for 5 seconds and delay for 30 seconds, thus replicating Heider's timing. In the long-term condition, presentation was for 1 minute and delay for 30 minutes. The array and probes used were those developed in Experiment II.

Subjects were 24 students (12 men and 12 women; 21 undergraduates and 3 graduates) residing in a university housing unit (thus making feasible the laborious long-term experiment since the subjects were allowed to do as they wished during the 30-minute intervals). All subjects were tested in a small room (college dormitory decor), seated across a desk from the experimenter with the covered array on the white desk top. Lighting was bright, from multiple sources, and identical at the time of presentation and recall.

For each subject, the short-term condition was administered first and the long-term condition second. Each subject received 18 of the 24 probes in the short-term condition (6 focals and 12 nonfocals) and 6 of the 24 probes in the long-term condition (2 focals and 4 nonfocals); within that presentation frame, the chips were randomized for each subject with the restriction that all chips were exposed an equal number of times across all subjects. Filler time during the 30-second condition was occu-

pied by silence or, more usually, by idle conversation. In the long-term condition, where the filler time interval was 30 minutes, subjects were allowed to leave the experimental area and do as they wished. They had to return after 30 minutes to make an identification and to receive another presentation. At the conlusion of the final recognition trial, or on subsequent occasions, the purpose of the experiment was explained to those who cared to know, and the subjects were asked to comment on the experiment (e.g., difficulties, strategies, etc.).

Results: Table IV presents the accuracy scores for both memory conditions. As Heider notes, the basic measure in this experiment is the accuracy score (number, or percent, correct); problems with the latency score were mentioned in Experiment I. Again, the data were analyzed both by subject and by chip. In the short-term memory condition, the focals and nonfocals are *not significantly different* as measured by the "by-subject" analysis ($t_{23} = 1.52$, n.s.) or as measured by the "by-chip" analysis ($t_{22} = 1.01$ pooled variance, n.s., and $t_{14.7/21.5} = 1.03$ separate variance, n.s.)(see Table IV). Converting the pooled variance analysis to its associated correlation coefficient by the usual formula (see Welkowitz, Ewen, and Cohen 1971) of $r = \sqrt{t^2/(t^2 + df)}$, we derive $r = .21$ (n.s.). To compare the short-term accuracy results in Table IV with Heider's results in Table II, we need only use our percent correct figure to compute how many chips Heider's subjects would have gotten correct had they succeeded at the same rate as our subjects. To ease the comparison further, since there are twice as many nonfocal as focal probes, the nonfocal score can be scaled down by one-half (see Table V).

In the long-term memory condition, a significant difference between focals and nonfocals emerges for the first time in the "by-subject" analysis ($t_{23} = 2.64$, $p = < .05$). However, the two "by-chip" analyses are not significant ($t_{22} = 1.45$ pooled variance, n.s. and $t_{14.7/21.5} = 1.86$ separate variance, $p = < .10$) (see Table IV). Converting the pooled variance analy-

TABLE IV. Accuracy Scores (Percentage Correct) for Recognition Memory Experiment (Experiment III)

	Short-term memory	Long-term memory
$\bar{X}$ focal	62.5	58.3
$\bar{X}$ nonfocal	53.7	41.6
Three t-test analyses of focal/nonfocal differences		
t_{23} (by subject—24 paired scores)	1.52	2.64
p (two-tailed)	(n.s.)	$< .05$
t_{22} (by chip-pooled variance)	1.01	1.45
p (two-tailed)	(n.s.)	(n.s.)
$t_{14.7/21.5}$ (by chip-separate variance)	1.03	1.86
p (two-tailed)	(n.s.)	$< .10$

TABLE V. Comparison of Short-Term Memory Scores in Experiment III and in Heider (1972)

Accuracy (Number correct)	Heider	Experiment III
$\bar{X}_f$	5.25	5.00
$\bar{X}_{nf}$	2.87	4.30

sis to its associated correlation coefficient by the method discussed earlier, we derive $r = .29$ (n.s.).

A separate t test (paired scores) indicated that the long-term condition was significantly more difficult ($t_{23} = 5.15$, two-tailed $p = < .001$) than the short-term condition. . . .

Discussion: Looking at the short-term memory results, we see that Heider's finding of superior memorability for focals *is not replicated* once the perceptual discrimination for the two categories of chips is equalized. The comparisons in Table V allow us to see that the major difference between our findings and those of Heider resides in the markedly improved accuracy scores for the nonfocals. This suggests that the main effect reported by Heider is because of the superior discriminability of the focals in the array and not to any "saliency" inherent in the colors themselves. Once the discriminability of the nonfocals is brought up to the level of the focals, the recognition scores become roughly equivalent also. The focal chips in Heider's array profit in recognition memory from superior discriminability from their neighbors.

Under the long-term memory condition, the significant advantage of focals in accuracy scores expected by Heider does emerge on the unbiased array (although in a marginal way). However, the relationship of focality to codability and codability to recognition memory remains to be analyzed in Experiments IV and V.

In an attempt to discover what factors might be involved in the memory task, clues were sought in the comments and incidental behaviors of the subjects. Three methods emerged as the dominant strategies among our subjects.

One strategy was basically verbal. Subjects generally labeled the color chips with one of the primary color terms (e.g., "red," "blue") and then supplemented this with other stored information to distinguish the particular variant at hand. One method of supplementation involved simply storing "visually" the idiosyncratic aspects of the chip. It is noteworthy that few subjects reported using pure visualization without the verbal element. Two who did (one reporting that she tried to get the "feel" of each color) were among the most accurate. A second method of supplementation was to append to the primary term a more involved verbal modification (e.g., "pea-soup green but a little lighter"). Different subjects made these added descriptors more or less complex; some subjects varied their own complexity in light of their general memory for how many similar confusable colors they recalled seeing in the array. Thus, for exam-

ple, knowing that there were many greens and few reds, the subjects would make more complex descriptions for the greens than for the reds. These latter subjects were among the most accurate. An interesting aspect of this strategy (emerging especially under the long-term condition) was the disastrous effect of forgetting the primary term. Without memory for the basic term (e.g., "it was a blue"), subjects had absolutely no idea which color, or type of color, they had seen. Subjects just stared hopelessly at the array. Subjects who had the primary term were quite comfortable and much less often had a sense of not remembering the specific chip, even when they were quite far off in their matches.

A second strategy was to relate the color to some colored object stored in permanent memory (e.g., "color of my mother's lipstick," "color of my bedroom"). These objects were often verbally encoded, and it appeared that the verbal name for the object was used in memory. Subjects using this strategy, then, were often using a verbal code to elicit a stored object memory from which they derived the needed color. The approach seemed distinct from working completely within a semantic color system and from using free-floating visualization. Subjects apparently felt fairly confident that these stored object colors were reliable for repeated comparison as required in the memory experiment. In many cases, the matches to famous colors (e.g., "Kodak yellow," "Pepto-Bismol pink") were quite compelling in their aptness.

A final strategy involved visually matching the probe with some object present in the room (e.g., an article of clothing). Then, at time of recall, the subject would try to find a color in the array that was either identical to the object or that differed in some way from the object that they had also stored (perhaps verbally) (e.g., "a little bluer than the green of my shirt"). Under experimental conditions, that strategy is not terribly helpful and was not frequently employed. Whenever a subject did mention using this method en route, the use was noted: more often than not, the subject made an incorrect selection. Nonetheless, this is undoubtedly one of the more common everyday techniques for remembering a color—similar to taking a swatch of cloth to the store to match better a dress, curtains, upholstery, etc. Experiments conducted under highly artificial conditions (that is, with all-neutral room and clothing) or without careful observation and questioning of subjects might not detect this strategy. (Note that, for focals to outperform nonfocals with this strategy, they would have to be more available in the environment.)

If we conceive of a color as a bundle of information to be stored, the subjects appear to utilize a "standard" (a verbal code, a remembered object, a present object) to encode in memory the bulk of the information. Then they only need to actively store the much smaller quantity of information necessary to distinguish the color at hand from the standard. We may, after Stefflre, regard memory to be much like communication except that the information is not conveyed from speaker to hearer but from time of memory to time of recall/recognition. Since conventional forms (culturally organized natural language) are dominant in interpersonal communication, it is natural to ask to what extent such conventional forms are important in intrapersonal communication, that is in memory.

That would amount to assessing the extent to which each of the three standards is likely to be conventional. Present objects are not likely to be conventional as the strategy emerges here. Remembered objects are quite likely to have a normative aspect although whether we can call that conventional or not is open to question. A verbal code, of course, would immediately implicate conventional forms. Further, verbal supplements are apparently used in conjunction with the other two standards as well. In short, there is good reason to suppose that the search for an influence of language on memory for color would produce positive results in light of our subjects' reports. Experiments IV and V explore that possibility.

Before leaving our analysis of subjects' self-reports, two more items deserve mention although they are not taken up further in what follows. First, subjects had strong emotional or subjective reactions such as "ugly," "pretty," "dull," etc. to specific colors. Further, many subjects responded to the color-memory procedure as if it were a test of intellectual or artistic giftedness. That is perhaps understandable given the university student population and the general association of psychology with abilities testing. It is not clear to what extent such emotional and motivational factors may influence the experimental results.

Second, some aspects of subjects' decision making are worthy of note. Subjects did not appear to be more likely to be correct in their selections simply because they felt "sure" of the color. No evidence of a reliable relationship between feelings of "sureness" and performance was evident in accuracy although subjects did tend to respond more quickly. It was also very common for subjects to narrow the field to two colors as possible correct choices and then to select the wrong one of the two. It may be that a more sensitive measure, such as one indexing perceptual distance of the selection from the correct choice or asking for a second choice, would produce more interesting results than a simple correct/incorrect scoring. (An approach using that notion in a different setting is undertaken in Experiment V.)

Experiment IV: Memory and Two-Person Communication Accuracy

Purpose: This experiment explores the possible influence of linguistic encoding on memory for color. The linguistic measure employed is the accuracy of communication using the color chips in a two-person communication task. The specific goal is to see whether an index of this sort correlates with memory performance and, in particular, whether it is superior to focality as a predictor of such performance.

Method: The basic design involved having one subject (an encoder) describe verbally a color chip so that a second subject (a decoder) could select the chip from the array. Encoder and decoder sat on opposite sides of a table with the array between them. The experimenter indicated the target to the encoder, while the decoder turned away for a moment. Then the encoder had up to five opportunities to describe the chip in such a way that the decoder could locate it in the array. Encoders turned

away while decoders indicated their choices. Encoders were instructed not to use locative descriptions and not to make references to any objects of similar colors in the room. The target chips and array were those used in Experiments II and III. A record was kept of the number of trials necessary for successful location of each chip; in some cases, a record was also kept of the verbal descriptions used. Twenty-two pairs of subjects were tested.

Results: Since we were interested not in a dichotomous variable (such as focality) but in a hypothesized continuum of communicability, correlation coefficients were used to evaluate the results. These correlations have the advantage of indicating the strength of the relationship as well as its significance. Such an index of relative strength will be important in comparing the various predictors of memorability.

Two scores were computed for each chip: one based on the number of times the decoder selected the correct chip on the first try (number of first hits) and the other on the mean number of tries necessary for identification (with a maximum of 5) across all 22 pairs. Each of these communication accuracy scores is significantly correlated with the short-term and long-term memory accuracy scores from Experiment III, with correlations ranging from .54 to .58. These figures are presented in Table VI. Since, even in our equalized array, a spurious correlation might have existed between communication accuracy and perceptual search accuracy (see Experiment II), partial correlations were computed for each accuracy value controlling for this factor. In no case in any of our experiments did the partial correlations substantially reduce the size of our original correlations or change the significance values. . . .

Discussion: Whereas focality did not relate at all to short-term memory in Experiment III, communication accuracy does in Experiment IV. In fact, in both memory conditions there is a significant relationship between communication accuracy and recognition memory. (Of the two measures of communication accuracy, first hits are the more valid in comparison with memory strategies and performance since in the memory task, second choices were not elicited.) This suggests either that language itself is involved in memory or that the factors that influence communication accuracy also influence memory. The significance of that finding is discussed after we present the following results of Experiment V.

TABLE VI. Two-Person Communication Task: Correlation Coefficients (R) Between Measures of Two-Person Communication Accuracy and Memory Accuracy (N = 24 Chips)

Communication accuracy	Short-term memory	Long-term memory
First hits	.58***	.54**
Mean tries (−r)	.54**	.58***

** two-tailed $p < .01$
*** two-tailed $p < .001$

EXPERIMENT V:
MEMORY, GROUP COMMUNICATION ACCURACY, AND REFERENTIAL CONFUSABILITY

Purpose: In Experiment III, subjects utilizing a linguistic description in memory commented that much of their difficulty in remembering certain chips had to do with the greater number of alternatives in the array for certain hue names. For example, an encoding such as "light green" might be applicable to many more chips in the array than an encoding such as "dark orange."[4] More formally, a single linguistic description used in memory can have several appropriate referents at the point of recall. It was our hunch that the linguistic descriptions typically used by subjects would vary in the range of their applicable referents in the array and that this difference in range, the degree of *referential confusability,* would be highly correlated with the memory results of Experiment III. Experiment V investigates that possibility. Further, by a slight addition to the procedure, we are able to get an alternative measure of communication accuracy. This new measure of communication accuracy is derived from the descriptions typically used by subjects and is therefore more clearly independent of the skills of individual encoders and decoders and the immediate context.

Method: The first step in assessing referential confusability was to find out exactly what descriptive phrases were used by subjects. To gather a collection of descriptions, 29 university subjects (15 men and 14 women) were asked to describe the target colors so that another person could identify the color if he had to select it from an (unspecified) array of colors. The only restriction on subjects was that they limit their descriptions to three words or less.[5] Having in hand this large collection of descriptions, some method was needed to derive from them a basic description for each target so that we could assess how many chips in the full array might meet the description. Alternative procedures, such as giving subjects each individual description to decode and computing mean values for each target, were deemed too tedious, given the 696 individual descriptions involved. Even some method of counterbalanced subsampling, such as that used by Lantz and Stefflre, would have required about 180 decodings for each subject, thus making the task extraordinarily lengthy and boring. Both of those methods also camouflage the linguistic variable at work.

Preliminary attempts to reduce the 29 descriptions available for each chip to a single basic description by simple mechanical procedures (e.g., adding up the number of usages of each lexical item) produced meaningless results. It became obvious that some account would have to be taken of the syntactic relations among the elements within a description and that a certain amount of subjective judgment would have to be exercised in certain instances (e.g., Are *army green* and *military green* the same? Does *pea-soup green* involve one modifier of *green* or two?).

The following procedure was used for the set of 29 descriptions associated with each chip. The last term of each description was taken as the basic term (e.g., if the description was *royal blue, blue* was the basic

term). Once the basic terms had been listed, the modifiers were examined. If there was no modifier (e.g., the subject simply said "blue"), then one point was scored for that basic term in unmodified form. If there was a single modifier (e.g., "royal blue"), then one point was scored for the basic term with that modifier. If two (or more) modifiers were present, a decision had to be made about whether the two formed a compound modifier (e.g., *pea-soup green*), scored as a unit, one point for a single modifier; formed two independent modifiers (e.g., *greenish turquoise blue*), scored one-half point each; or formed embedded modification (e.g., *light royal blue*), scored the same as without the outer modification (i.e., *light royal blue = royal blue*).[6] (In some cases, there was ambiguity (e.g., *deep sea blue* could be interpreted as *deep-sea blue* or as *deep sea-blue*), which was resolved case by case. In most instances, the scores that mattered were not changed.) *Any basic term-modifier combination accumulating four points or more was counted as a component of the chip's "basic description."* Many chips had basic descriptions with more than one such component. Once there was a basic description for each target chip, we assessed how many other chips in the array could meet each description. Ten university subjects (six men and four women) were presented with the basic descriptions and were asked to pick out the best exemplar of the description and any other chips in the array that could be accurately described by the description. Rows and columns in the array were numbered so that each chip in the array was uniquely identifiable. The task was self-administered by some subjects and experimenter-administered to others. Three scores were available: (1) the mean number of alternatives applicable to the basic descriptions for a target chip, that is, what S's considered to be "reasonable choices" ("number of reasonable choices"); (2) the absolute number of different chips considered the best exemplar of the basic description, that is, number of different chips across subjects selected as "best example" ("number of best examples"); (3) the number of times subjects correctly selected the original target chip as the best example of the basic description ("target as best example"). This third score may be considered an alternative measure of communication accuracy—what we will call "group communication accuracy." Notice that, unlike the two-person communication accuracy measure, our "group communication accuracy" indicator reflects the efficacy of basic descriptors developed from one group's cumulated descriptions in guiding a second group's cumulative decoding efforts. A little later we discuss the empirical relationship between the two-person and group communication accuracy scores.

Results: . . . The 40 component descriptions used in forming the 24 basic descriptions account for 272 encoder responses out of the total of 696—that is, 39% of all encodings were represented in the basic descriptions given to the decoders. If we take the component descriptions with the highest modifier scores from each basic description and use them as an index of intersubject agreement on a particular chip, we find that intersubject agreement (a measure of codability often used in the past) does not correlate with either short-term memory ($r = -.20$, n.s.), long-term

memory ($r = -.12$, n.s.), or focality ($r = .10$, n.s.). Thus, agreement on how a chip is best described does not assure high recognition memory.

The decoders selected the original targets 34% of the time in their indications of the best example of the basic description. This group-communication accuracy score correlated significantly with both short-term memory ($r = .59$, $p < .001$) and long-term memory ($r = .63$, $p < .001$) (see Table VII).

The mean number of reasonable choices for the basic descriptions ranged from 1.2 to 4.2. This score was correlated with the memory scores for the target chip that had been the original basis for the description. As predicted, these correlations were significant for both short-term memory ($r = .57$, $p < .01$) and long-term memory ($r = .48$, $p < .01$) (see Table VII). The number of different chips selected as best example provided an alternative index of referential confusability (that is, how many really close competitors there were—close enough to be selected as best example). The number of best examples ranged from two to eight and correlated significantly with both short-term memory ($r = .55$, $p < .01$) and long-term memory ($r = .69$, $p = < .001$) (see Table VII). However, this latter measure is independent with the number of correct selections of the target. . . .

Discussion: High intersubject agreement on basic descriptions is not in and of itself predictive of recognition memory. Nonetheless, the 34% success rate of the decoders indicates that distinctive information concerning the targets was maintained under the procedure for producing basic descriptions. This success rate compares favorably with the 40% success rate under the two-person communication task (where the array was available to the encoder) and with the 57% and 47% success rates under short- and long-term memory conditions respectively (where the decoders were working with their own personal memory encoding of the chip). That these subjects did so well with a standardized linguistic description formulated independently of the array is truly remarkable.

TABLE VII. Correlation Coefficients (R) for Measures of Group-Communication Accuracy and Referential Confusability with Memory Accuracy (N = 24 chips)

Group-communication accuracy		
Number of times correct target selected as best example	.59***	.63***
Referential confusability		
Number of different targets selected as best example (−r)	.55**	.69***
Number of reasonable choices (−r)	.57**	.48**

** two-tailed $p < .01$
*** two-tailed $p < .001$

Previous findings of a relationship between communication accuracy and recognition memory (e.g., Lantz and Stefflre 1964) have been regarded to be unduly dependent on the abilities of individual encoders (or decoders) or to be dependent on linguistic descriptions formulated specifically for the color array at hand. Notice that the group communication accuracy score escapes those problems and yet produces the same general pattern of results as the more traditional two-person communication accuracy procedure. The correlation between the two measures of communication accuracy is substantial ($r = .81$, $p = < .001$). Previous findings have also been criticized because the color arrays utilized lacked focal chips; both of our experiments use an array with focal chips. Communication accuracy proves to have a far stronger relationship to recognition memory than does focality.

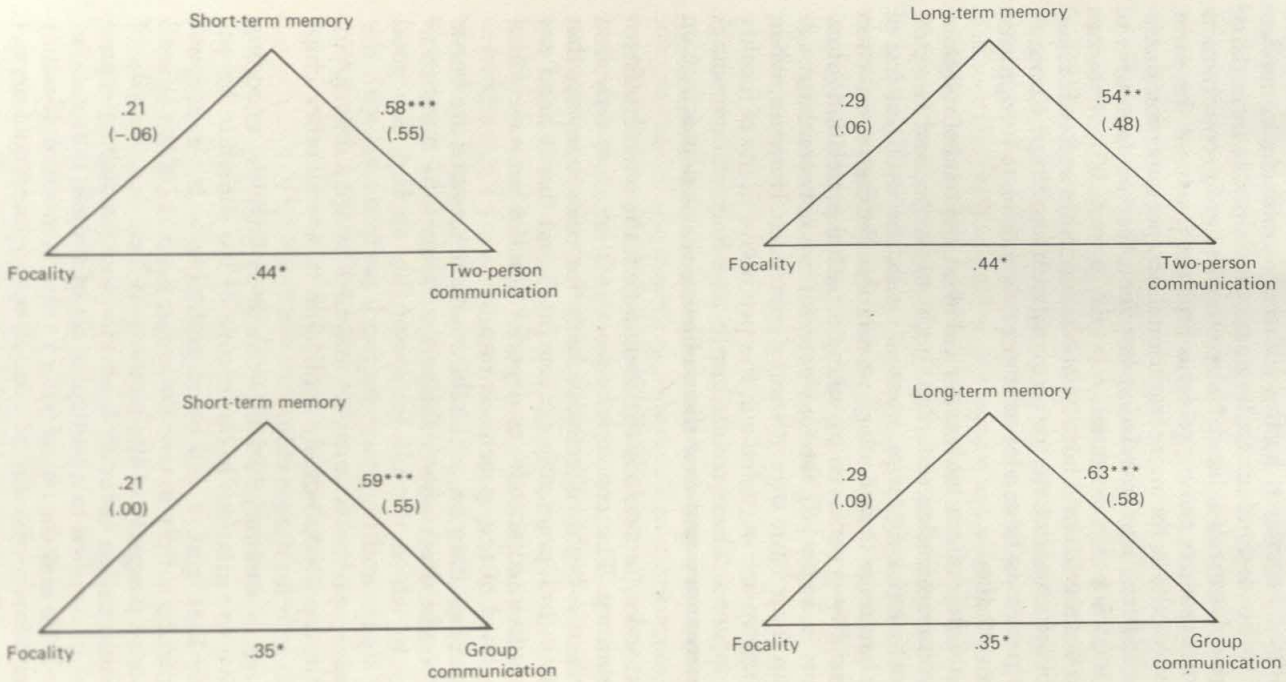
Figure 2 presents, in pictorial and numerical form, the triad of relations among focality, communication accuracy (both measures), and recognition memory (short- and long-term). In every case, the strongest leg of the triangle is the language-and-memory correlation. Further, even when the influence of focality is removed by taking a partial correlation (numbers in parentheses in Figure 2), the four correlations between language and memory retain their basic strength and significance. However, when the linguistic influences are partialed out, the predictive utility of focality reduces to close to zero. These results imply that linguistic encodings are used in color memory and that their efficacy is not dependent on focality.[7]

It would be a mistake to conclude that language is the only significant factor in color memory. The conclusions we stress here are only that language can serve as a highly effective vehicle for color memory, that it appears to operate independently of "focality," and that it need not be dependent on individual encoder or decoder abilities but may reflect regularities at the level of the speech community.

Our expectations regarding the confusability of a description are borne out: the size of a basic description's referential range (the number of chips in the array to which it could be reasonably applied) is a good predictor of both short- and long-term memory performance with the encoded chip. That is reasonable since, no matter how apt a description is, it will be of little use if it is equally applicable to several other chips or more applicable to a nontarget chip.

In actual practice, a memory technique such as linguistic encoding is probably adaptive to variations in the nature of the stimuli to be encoded; thus, in the long run, it will be unprofitable to try to account for subject performance simply in terms of some property of the stimuli (e.g., focality) or some property of the memory vehicle (e.g., brevity of description). Communication accuracy is an effective predictor because it reflects *language as applied* to a particular set of stimuli, thus encompassing both the vehicle and the object within a single measure. If either the linguistic descriptions or the stimuli were changed, one would expect the communication accuracy scores (and by implication the memory scores) to change also (cf. Lantz and Stefflre 1964).

To indicate the operation of these linguistic variables in a more con-



*Two-tailed $P < .05$

**Two-tailed $P < .01$

***Two-tailed $P < .001$

FIGURE 2. Relationship between language indicators and focality to short- and long-term memory accuracy with the language variable or focality partialed out (in parentheses) (N = 24 chips).

crete way, it will be useful to examine in greater detail three of the most telling cases.

Chip 8 (a nonfocal) has the basic description "flesh/peach." Neither word is a "basic" color term in English; neither is the chip particularly perceptually salient in Heider's terms with its saturation level of 6, nor is it likely to be universal across languages, cultures, or races. Yet, this color was the single best-remembered chip in long-term memory and tied with three other chips as second best-remembered in short-term memory. Clearly this nonfocal color is well remembered. Our linguistic measures detail the potential sources of its strength. It has the most-agreed-upon basic description of any chip; this description has the second-fewest competing reasonable choices and the second-fewest number of chips selected as best example. The success of this chip apparently lies in its cultural and linguistic saliency to Americans (conventionalized by the Crayola Company) and in the lack of competition for its basic description in the array.

Chip 14 (also a nonfocal) has the basic description "light yellow/pale yellow." This color chip is not salient in terms of its saturation, which is low (six), nor would its description be cited as a basic term of English although it contains such a term in "yellow." Unlike "flesh," however, the description does not have any obvious cultural or racial significance and has only moderate intersubject agreement. Nonetheless, this chip is the best remembered of any chip under short-term memory and ties for second-best-remembered in long-term memory. Our measures reveal that its strength lies in the lack of competition in the array for this description. It has the highest number of correct choices of the original target as best example; it also has the fewest number of chips listed as reasonable alternative choices.

Finally, for contrast, we can look at Chip 22 (a focal) with the basic description "pink." Although the chip has low saturation (six), it is one of the focal chips and has as its description a basic color term on which there was high intersubject agreement. Despite those attributes, it has the second-worst score on short-term memory and is easily the worst-remembered focal on either long- or short-term memory. Its basic description exhibits a high number of reasonable alternative choices in the array and led only one subject to the original target. (The other subjects distributed their choices for best example among four other chips). Despite high intersubject agreement and a target that was a focal, this chip simply suffered from too much competition in the array—too many chips to which the description "pink" applied.

GENERAL DISCUSSION—WHORF AND THE COLOR RESEARCH TRADITION

Our examination of the empirical basis for the reversal of opinion in the color-research tradition finds that empirical base deficient. Reviewing the literature, we find no direct contrast of the two views. Reanalyzing Heider's critical experiment, we find it to be based in part on artifact, and nonreplicable in the short-term memory condition. Conducting a

direct contrast of communication accuracy and focality as predictors of recognition memory, we find the linguistic measures to be far superior and the predictive value of focality to be near zero once these linguistic measures are taken into account. Given this evidence, the view of the earlier era regarding the importance of language in recognition memory seems more sound.

Further, we have introduced new evidence regarding the role of language as a factor in human color memory. Subjects' introspective accounts indicate that language is one type of "standard" used in memory and it is a useful supplement to other standards, such as permanently stored objects and images. Our second measure of communication accuracy indicates that this "standard" need not be idiosyncratic. Finally, it appears that the referential range of the linguistic description used is the critical element affecting performance. On balance, it seems that the metaphor forwarded by Lantz and Stefflre, that memory is analogous to intrapsychic communication with oneself over time, is especially apt in that the vehicle for this communication is often linguistic and in that memory success relates to the referential precision of the linguistic description at the time of recognition (decoding). We have also suggested how other vehicles for memory may operate in a similar fashion; certainly, language is not the only factor involved. To the extent that language *is* involved, however, it remains likely that different languages will induce different patterns of recognition. That conclusion is certainly supported by Stefflre, Castillo Vales, and Morley (1966) and by our evidence of group-communication accuracy.

It remains for us to reassess the relationship of color research to Whorf's original conception of the relationship between language, thought, and external stimuli. There are many ironies to be found in the color-research tradition. One is that color researchers (and others) have assumed that Whorf's linguistic relativism is incompatible with the existence of sublinguistic universals such as focality. It is ironic because Whorf not only knew better but explicitly cautioned against that misleading assumption:

> There is a universal *Gefühl*-type way of linking experiences, which shows up in laboratory experiments and appears to be independent of language—basically alike for all persons.
>
> Without a serial or hierarchical order in the universe it would have to be said that these psychological experiments and linguistic experiments contradict each other [1956:267].

Whorf was referring to universal phonetic symbolism, the sublinguistic universal receiving attention in his day. Certain nonsense sound patterns, e.g., "queep," seemed to elicit a universal set of associations, e.g., sharp (versus dull), quick (versus slow), hard (versus soft), bright (versus dark), etc., regardless of one's language or culture.

Whorf did not deny that phonetic symbolism might be a sublinguistic universal, nor did he feel he had to choose between the implied nativism of the phenomena and his own linguistic relativism. He did not have to choose because, from Whorf's perspective, the relationship between biological universals (what he referred to as "lower-psyche facts") and

language and culture was not one of contrast but rather one of "appropriation." He points out that "There is a yogic mastery in the power of language to remain independent of lower-psyche facts, to override them, now point them up, now toss them out of the picture . . ." (Whorf 1956:267). In the case of phonetic symbolism, this means that any language is free

> . . . to mold the nuances of words to its own rules, whether the psychic ring of the sound fits or not. If the sounds fit, the psychic ring of the sounds is increased [and is noticed, as in poetry]. . . . If the sounds do not fit, the psychic quality changes to accord with the linguistic meaning . . . [Whorf 1956:267].

As an example, Whorf pointed to the sound pattern "deep." Phonetically, "deep" is akin to "queep"; speakers of languages other than English associate "deep" with sharp, quick, bright, hard, etc. But "deep" is not meaning free to anyone reading this essay; its meaning overrides our sublinguistic affective sensitivity to its phonetic pattern. As Whorf remarks, "[the] linguistic meaning [of 'deep'] in the English language happens to refer to the wrong sort of experience for such an association. This fact completely overrides its objective sound, causing it to 'sound' subjectively quite as dark, warm, heavy, soft, etc. as though its sounds really were of that type" (268).

Once Whorf's hierarchical view is acknowledged (see 1956:267–268; see also 1956:239), it becomes possible to interpret more properly those paragraphs in his writings, which are so widely quoted yet so frequently misunderstood, in which he introduces his

> principle of relativity, which holds that all observers of the universe are not led by the same physical evidence to the same *picture* of the universe unless their linguistic backgrounds are similar, or can in some way be calibrated. . . . The categories and types we isolate from the world of phenomena we do not find there because they stare every observer in the face; on the contrary *the world is presented in a kaleidoscopic flux of impressions which has to be organized by our minds*—and this means largely by the linguistic systems in our minds. We cut up nature, *organize it into concepts,* and ascribe significances as we do, largely because we are parties to an agreement to organize it in this way—an agreement that holds throughout our speech community and is codified in the patterns of our *language* [1956:214–215; emphases added].

What Whorf says in these remarks is that the world *presents* itself in a "kaleidoscopic flux of impressions." Nowhere does he suggest that the world is actually *perceived* as a continuum; he acknowledges that the world is perceived categorically and by reference to types. Whorf's point is a *logical* one; from the point of view of the way the world *presents* itself, all things are equally alike and equally different, that is, the number of true things one can say about any two things (the number of predicates that apply) are equal, and perhaps infinite. This notion of logical (or metaontological) equidistance has been more recently discussed by Goodman (1968, 1972) and Watanabe (1969:376–388; also see Shweder, Bourne, and Miyamoto 1978). Watanabe provides a formal proof that "insofar as the number of shared predicates is regarded as a measure of similarity

and the number of predicates that are not shared is regarded as a measure of dissimilarity . . . there exists no such thing as a class of similar objects in the world," that is, "the world would look 'gray,' amorphous, and meaningless. . . ." Or, as Goodman (1968:32) remarks, "to admit all classifications on equal footing amounts to making no classification at all."

Echoes of Whorf can be found throughout Goodman's (1968, 1972) discussions of "representation." Notice that Whorf's primary concern was with the relationship between our "picture" of the world and the world as it presents itself. What he suggested throughout his writings were the principles that: (a) there is no such thing as a single copy of the way the world actually is; and (b) the relationship between a representation (e.g., a "picture") and the thing represented (e.g., the "universe") is not one of resemblance. Both principles have been recently defended by Goodman (1968:6), who remarks:

> "To make a faithful picture, come as close as possible to copying the object as it is." This simple-minded injunction baffles me; for the object before me is a man, a swarm of atoms, a complex of cells, a fiddler, a friend, a fool and much more. If none of these constitute the object just as it is, then none is *the* way the object is. I cannot copy all these at once; and the more nearly I succeeded, the less would the result be a realistic picture.

To those principles Whorf adds a third: that the way we group things into categories has its most important source in our particular speech community or culture.

In the passages quoted earlier, Whorf refers to our "linguistic" background (or "language"), on the one hand, and the way we categorize or organize nature into "concepts," on the other hand. He does not suggest that the relationship between language and categorization is to be thought of as the relationship between an independent and dependent variable (although that is the way this passage is often interpreted). Quite the contrary, there is good reason to believe that Whorf viewed the relationship between a people's language and their category system as the relationship of a whole to a part. For example:

> . . . language first of all is a classification and arrangement of the stream of sensory experience which results in a certain world-order. . . . [and] . . . every language is a vast pattern-system, different from others, *in which* are culturally ordained the forms and categories by which the personality not only communicates, but also analyzes nature, notices or neglects types of relationships and phenomena . . . [Whorf 1956:55, 252; emphasis added].

In other words, an adequate description of a language must include an account of the way a people classifies its world; ontology is a cultural inheritance reflected in the way members of a speech community talk to one another.

In short, then, Whorf did not deny the existence of sublinguistic universals (although he thought language "free" to ignore them, override them, or make use of them), nor did he claim that the world was perceived as chaotic (although, on logical grounds, he believed it must be considered as presented in that way), nor did he try to define language independently

of a culture's category system. It is in the light of those three points that the true ironies of the color-research tradition can be appreciated.

In the early era of research on language and thought, color was emphasized because of its supposed freedom from intrinsic organization (it was seen as a continuum). Even if that were true (and we have a long line of evidence indicating that it is not), it would be an unnecessary restriction since, from Whorf's perspective, any sublinguistic structure that exists could be used by language as it will. In the latter era, the perceptual irregularities in the color spectrum were seized upon to demonstrate the influence of sublinguistic universals on language. That position was seen as a critique of Whorf's assertions about a "kaleidoscopic" universe, which we now see were misinterpreted. Whorf's image of a "kaleidoscopic flux" is based upon a principle of *logical* equidistance; it is not to be analogized to a psychophysical continuum. For Whorf (and Goodman 1968 and Watanabe 1969 as well) two Munsell color chips are no more like each other than focal red is like the Eiffel Tower; that is, from a logical point of view, the number of true things that can be said about each pair (the number of attributes shared) is equal and infinite.

A second irony of the color-research tradition is that what Whorfian wisdom there was in the early era was subsequently ignored. The communication-accuracy measure introduced by Lantz and Stefflre, for example, was based on a view of language broad enough to include a culture's entire system of color categories, not simply their *basic* color *terms*. If one remains committed to this Whorfian conception of language, it becomes possible to draw two conclusions about the relationship among language, thought, and external stimuli in the color domain. First, a linguistic measure (communication accuracy) is predictive of recognition memory for color regardless of array, and, as we have shown, this applies to arrays containing focals as well as to those studied by Lantz and Stefflre. Second, the claim that the sublinguistic universal "focality" exercises a causal influence over language is quite tenuous. On the one hand, most color categories (e.g., blonde, sky blue, lavender, etc.) have no reference to focal areas at all; focality is not a necessary condition for forming a color category. On the other hand, focality is not a sufficient condition for the development even of basic color terms; cultures that have had equivalent amounts of time to evolve a color lexicon vary enormously in the number of basic color terms (not to be confused with color categories) in their linguistic repertoire. Berlin and Kay (1969) have made a significant contribution to our understanding of the evolution of basic color terms; nonetheless, their work was not intended to illuminate (and it does not illuminate) the way people make use of all their linguistic resources to encode color stimuli in practical cognitive tasks such as recognition memory.

What we discover in our various experiments on language, focality, and memory is consistent with Whorf's hierarchical view of the relationship between language (including cultural category systems) and sublinguistic universals. While focality may occasionally emerge as an important variable in the organization of behavior, the availability of a referentially precise basic color description (e.g., "flesh," "pale yellow") and not focality

is the decisive element in recognition memory. One is certainly free to appropriate linguistically the perceptually salient properties of focal chips (e.g., "the reddest red"), and there is a tendency for focal chips to be easier to describe than nonfocal chips (see Figure 2). Nonetheless, there are many influences other than focality on the ease with which a referentially precise description can be formulated, and the influence of focality on recognition memory is mediated almost entirely by basic color descriptions, a linguistic factor.

In summary, language appears to be a probable vehicle for human color memory, and the views developed by Whorf are not jeopardized by the findings of any color research to date.

NOTES

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1. Cross-cultural research on both color perception and color lexicons extends, of course, back to the turn of the century (e.g., Woodworth 1910, Rivers 1901) although that work has not been very influential in contemporary research. A variety of more linguistically oriented studies conducted during the 1950s considered the variability in color lexicons without directly assessing any cognitive variable (such as memory) (e.g., Conklin 1955) or merely assessed referential range (e.g., Ray 1953).
2. "Basic" color terms are a subset of the expressions for denoting color. According to Berlin and Kay (1969:6), the subset is identified by a number of criteria. Basic color terms must be monolexemic (blue is basic; sky blue is not), must not be included in the signification of any other color term (red is basic; crimson, a "kind of" red, is not), must not be restricted in their application to a narrow class of objects (yellow is basic; blonde is not), and must not be the name of an object having that color (yellow is basic; gold is not).
3. Several considerations favored this method of equalization. This array retains the majority of the chips in Heider's array, thus facilitating comparison; it randomizes the possible effects of immediate neighbors; and it shows behavioral evidence of successful equalization. Alternative methods of equalization include numerical weighting and construction of a new array in which chips are perceptually equidistant. Numerical weighting, as conducted by Heider, failed to prove effective since significant perceptual differences were found in Experiment I. Further, in weighting to compensate for accidental influences because of neighboring chips, real effects might have been obscured. Construction of a completely new array is not only very tedious but would have eliminated most of the focals from the array since they are extremes and cannot be made perceptually equipotent. This would also have destroyed comparability with Heider's array. On balance, then, the procedure of controlled modification of the existing array with direct behavioral assessment of focal/nonfocal equity seemed the best approach.
4. A rough preliminary count on our part indicated that four of the eight basic color terms of English (blue, green, purple, pink) were applicable to 75% of the chips in the array whereas the remaining four terms (brown, yellow, red, orange) were applicable to only 25%. This inequity might not be a problem if it could be shown that it represented the actual distribution of colors in

the natural world or if the distribution of the probes were proportioned in a similar manner. We cannot ascertain the actual distribution of colors, but we can examine the applicability of the eight terms to the probes; here, we find that the nonfocal probes, with their 81/19 division, approximate the 72/25 division in the array, but that the focals, with their 50/50 division, do not. This means that the focal set contains a disproportionate number of easy-to-identify chips if the subjects used basic color terms in memory, with their attendant potentials for referential confusability. For example, there are five nonfocal probes that might be termed "green," in contrast to one focal probe; "green" is one of those terms with many alternates in the array; thus, a subject's nonfocal accuracy score is likely to be lower simply because he must deal more than twice as often with the large number of alternative "greens" in the nonfocal set of probes.

5. This restriction was not imposed on the first few subjects. When it became clear that the overwhelming majority of the descriptions were three words or fewer, the three-word limit was then imposed on subsequent subjects to facilitate scoring.
6. This scoring of embedded modifications makes sense in light of the alternatives. For "light royal blue," giving one point to *light* and one to *royal* effectively equates the occurrence of *light* in this case with that in "light blue," which is misleading. Splitting the one-point total into one-half for *light* and one-half for *royal* is no better for it demotes this *royal* relative to the *royal* of simple "royal blue," and there are no grounds for that since the embedding implies the primacy of *royal blue* as the basic description. Considering *light-royal* as a compound with a joint value of one point also obscures the syntactic primacy of *royal* and, as a practical matter, dramatically eliminates modifier matches of any sort in the 24 targets. It is worth noting that this procedure tended to eliminate mostly such all-purpose modifiers as *light, dark,* and *medium.* (Very frequently, different subjects would use the opposite terms on the same color!) Other modifiers eliminated from descriptions were of much lower frequency but were of the same type: *pale, bright, very, deep, almost, sort of.*
7. In our examination of linguistic indicators, we were concerned with individual chips; we wanted to know whether the most communicable chips were also the most memorable. Thus, for the sake of comparability, the focality-memory correlation coefficients were also calculated "by chip." The most generous interpretation we can give to the relationship between focality and memory is to convert the "by-subject" t-test results in Experiment III into their associated correlation coefficients by the method discussed earlier. By doing that, we lose comparability with the linguistic indicators, but it is worth presenting the results nonetheless. r(focality—STM) = .30 (n.s.) (controlling for two-person communication accuracy, it reduces to .06). r(focality—LTM) = .48 ($p = < .05$) (controlling for two-person communication accuracy, it reduces to .32). Even under these generous conditions, the relationship between the linguistic indicators and memory is more substantial. r(two-person communication accuracy—STM) = .58 ($p = < .001$) (controlling for focality, it reduces to .52. r(two-person communication accuracy—LTM) = .54 ($p = < .01$) (controlling for focality, it reduces to .41).

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8

Salience and Relativity in Classification

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Studies in ethnoscience have led to the discovery that some form of taxonomic hierarchy is basic to man's classification of the biological world[1] (Berlin 1976; Berlin, Breedlove, and Raven 1973; Bulmer and Tyler 1968; Conklin 1954; Kay 1971). Any number of semantic dimensions, such as those of medical, spiritual, or nutritional significance, may serve as the basis for such taxonomic organization. However, hierarchies based upon relationships in morphological structure are consistently represented among the biological classification systems of diverse human groups, generally coexisting with various functional and otherwise nonmorphological hierarchies (Berlin, Breedlove, and Raven 1973; Anderson 1967; Bulmer and Tyler 1968; Diamond 1966; Hunn 1973, 1975; Wyman and Harris 1941). Cross-cultural comparisons of morphological classification have led to the recognition of a number of formal principles of taxonomic classification universally applicable to folk biological taxonomies (Berlin, Breedlove, and Raven 1973; Kay 1971). These formal principles of taxonomy have been found to correlate with linguistic, psychological, behavioral,

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† Source: *American Ethnologist* 5, pp. 66–80, 1978.

and ontogenetic data (Berlin, Breedlove and Raven 1973; Chambers n.d.; Dougherty n.d.; Hunn 1975; Rosch et al. 1976; Stross 1973), giving rise to speculations on the relative salience of the various levels of inclusion or taxonomic ranks within folk biological hierarchies (Berlin 1976; Berlin, Breedlove, and Raven 1973; Rosch et al. 1976).

Investigating the relative salience of categories of folk classification among the Tzeltal Mayans, Berlin, Breedlove, and Raven (1973, 1974) find folk genera to be the most salient conceptual groupings of organisms, and they generalize this finding to all folk biological taxonomies. They suggest that on the basis of a coupling of taxonomic and linguistic criteria with behavioral criteria such as frequency of use, ease of recall, gestalt recognition and primacy in development, the folk generic distinctions represent the fundamental units of any ethnobiological classification system (1973:240).

A series of experiments conducted by a group of psychologists (Rosch et al. 1975, 1976) was designed to reveal correlates of the most salient category cuts in folk systems of classification among urban Americans. It was found, as above, that "there is one level of abstraction at which the most basic category cuts are made" (1976:382). The basic categories for these authors are those that carry the most information and possess the highest category cue validity by virtue of the convergence of maximal values for a series of perceptual and behavioral dimensions at one classification level. The distinctions made at this level are the most differentiated from one another; and, they suggest, the most salient to the human perceiver. In the case of biological hierarchies they find basic category distinctions are consistently made by their sample of the American population at levels superordinate to the folk genera.

This discrepancy in the most salient category distinctions of biological domains for diverse populations will be considered below. It will be argued that no single taxonomic rank can be assumed a priori to be fundamental to the hierarchy in which it is embedded, and that the most basic categories in any given domain may range over a considerably larger number of taxonomic ranks than previously suggested. The fundamental taxonomic rank in a classification system will be seen to depend primarily upon the overall salience or cultural significance of the domain for some particular group of people. This implies that human perceivers tend to see organisms as representative of more or less inclusive categories. What the urban American classifies as a *tree* at first glance, the Tzeltal Mayan may classify as an *oak*. Further, the most salient categories for individual members of any society will be shown to vary along the same dimensions identified as significant determinants of cross-cultural variability.

The overall salience for any semantic domain will be seen to be reflected in the evolutionary status of the taxonomic classification system for that domain. It will be argued that there is a shift in the most salient taxonomic rank of a hierarchy toward increasingly more inclusive levels as the overall salience of the domain itself declines and the taxonomic structure devolves. The most basic or salient level will be shown to be a variable phenomenon shifting primarily as a function of general cultural

significance and individual familiarity and expertise, and secondarily as a function of perceptual homogeneity, objective correlational structure, or the degree of internal variation in the domain membership.

GENERAL PRINCIPLES OF TAXONOMY

A number of general principles of taxonomic organization have been discovered that appear to have widespread relevance for folk classification (Berlin, Breedlove, and Raven 1973). Of particular importance has been the recognition of six ethnobiological ranks. The most inclusive rank is the unique beginner, or domain head, representing the general distinction between the domains of *plants* and *animals*. Immediately partitioning the unique beginner is a set of relatively few major morphological classes demonstrating a high degree of internal diversity. The categories *tree, bush,* and *vine* are representative of life-form distinctions.

Subdividing the life-form categories are the folk genera, which are the most numerous taxa of biological hierarchies. The folk genera are characteristically homogeneous or monotypic terminal taxa. The categories *aspen, oak, redwood,* and *monkey puzzle tree* are typical folk genera. A few morphologically distinct or aberrant folk genera are not included within any life-form class but are immediately included in the unique beginner. *Bamboo,* for example, is a folk genus frequently excluded from any life-form class. These folk genera can be distinguished from the life-form classes by the relatively little internal diversity in their membership.

In those cases where folk genera are polytypic, they are partitioned by folk specifics, such as *white oak* and *staghorn sumac,* which in turn may be subdivided by varietal taxa, such as *cutleaf (staghorn) sumac.* Folk specifics and varietal taxa are considerably less numerous than folk genera and tend to occur only within folk generic categories of high cultural significance.

The sixth ethnobiological category has been designated intermediate. Intermediate distinctions commonly occur between the life-form and folk generic levels.[2] They are uncommon distinctions in most folk biological taxonomies and have been treated as quasi-categories or temporary distinctions symptomatic of ongoing change. The distinction among *leaf-bearing, needle-bearing,* and *frond-bearing* trees is typical of intermediate distinctions.

This set of naming conventions for taxa and ranks, or levels, within taxonomic hierarchies, while not universally accepted (Conklin 1954; Bulmer 1974; Dwyer 1976), has been used in a large number of ethnoscientific investigations and will be adopted here.

In addition to the recognition of distinct ethnobiological ranks, the relations of contrast and inclusion (Kay 1971) are universally applicable to folk biological taxonomies and relevant to the following discussion. Of particular importance are the relations of (1) immediate inclusion, that is, the relation between two categories so that one includes the other and there are no intervening categories (*jack oak* is immediately included in *oak*), and (2) contrast set, that is, the complete set of categories immediately included in the same superordinate taxon (the set of kinds

of *oak*, including *white oak, red oak,* and *live oak* comprises one contrast set).

GENERAL PRINCIPLES OF NAMING

Correlated with the hierarchically organized ethnobiological categories are some general principles of naming behavior. It has been suggested that an excellent indication of a conceptually relevant grouping of organisms is the fact that it has an easily elicitable and widely recognized (standardized) name (Berlin, personal communication 1975). Further, within the class of named categories those distinctions most frequently referred to will tend to have relatively short, semantically opaque linguistic labels (Brown 1958:235–241). For example, a commonly distinguished category of trees is more likely to be called something like *elm* by almost all members of a community than to be labeled with either an ad hoc, nonstandardized construction like, "you know, those tall trees with asymmetrical, serrated edged leaves" (Frake 1962:30) or a standardized but complex, semantically transparent construction like "serrated leaf tree." This correlation between frequency of use and kind of linguistic name allows the nature of a linguistic label to serve as an indication of the relative significance of the category it designates.

The names of botanical categories can be distinguished as either primary or secondary lexical items (Berlin, Breedlove, and Raven 1973). Secondary lexical items are complex constructions that occur in contrast sets, all the members of which designate their immediately superordinate taxon in their own linguistic label. The following subset of folk specific distinctions within the category *oak* is exemplary: *Jack oak, post oak, blue oak, live oak, red oak, white oak.* All other category names are primary lexical items. Primary lexical items can be differentiated into simple monolexemic forms such as *oak* and *elm*, and complex multilexemic or multimorphemic forms such as *poison oak* and *monkey puzzle tree.*

A strong correlation has been observed in botanical classification between the taxonomic rank of a particular category and the linguistic nature of its name. Groupings of folk generic or life-form level specificity are (with predictable exceptions) labeled by primary lexical items, while less inclusive categories at the folk specific and varietal levels are (again with predictable exceptions) named by secondary lexical items (see Berlin, Breedlove, and Raven 1973 for an account of exceptional naming behavior). Because unique beginners and intermediate category distinctions are rarely named in folk classifications and because when they are named the shape of the linguistic designation is highly variable, no correlation is possible for these distinctions with naming behavior.[3]

This taxonomic and linguistic evidence suggests that all folk biological hierarchies are comprised of a series of categories organized in a series of levels or ranks related by inclusion and designated by specifiable kinds of linguistic labels depending upon their taxonomic rank. The patterning suggests that the folk genera, that is, the large set of monotypic taxa requiring primary lexical designations, are the fundamental categories

of ethnobiological classification systems. However, when behavioral criteria are considered it appears that psychologically the most salient level of an ethnobiological hierarchy does not consistently fall at the level identified as basic by taxonomic and linguistic criteria.

GENERAL PRINCIPLES OF BEHAVIOR

In the discussion to follow a distinction will be maintained between societies maintaining greater or lesser degrees of functional interaction with their biological environment. This distinction is operationalized as direct or indirect dependence upon the biological environment for subsistence. The contrast between the Tzeltal Mayan agriculturalists (high interacting/direct dependence) and urban Americans (low interacting/indirect dependence) that will be considered below is illustrative. The relative salience of a biological domain for some population is then a function of the degree of direct interaction with this domain as determined by extent of experience with the membership of the domain and the need to communicate about this domain membership.[4]

Comparative work among traditional peoples maintaining a high level of functional interaction with their botanical environment (Berlin 1976; Berlin, Breedlove, and Raven 1973, 1974; Conklin 1954; Wyman and Harris 1941; Robbins, Harrington, and Freire-Marreco 1916) suggests that the folk genera are psychologically the most salient category distinctions in the botanical domain as well as being taxonomically and linguistically most fundamental. The folk genera for such peoples appear to be the most commonly referred to groupings of organisms across all contexts of reference. Specifically in elicitation settings the folk genera are the most frequently named categories and the most readily recalled. One investigator notes that he was quickly provided with a separate, distinct name for what appeared to be each botanically distinct tree by an Aguaruna informant. He notes that from a resting spot on the Jungle trial, this Aguaruna informant easily listed the names (folk genera) for no less than forty distinct trees (Berlin, personal communication 1975). An early ethnobotany of the Tewa Indians similarly reports "they have a name for every one of the coniferous trees of the region" (Robbins, Harrington, and Freire-Marreco (1916:9). And Smith-Bowen remarks of the Tiv that "every plant, wild or cultivated, had a name and a use, and . . . every man, woman and child knew literally hundreds of plants" (Smith-Bowen 1964:19).

The tendency to rely on folk generic distinctions in naming behavior and daily interaction suggests that the most significant category distinctions are made at this level. Two additional observations reinforce this conclusion. The first concerns the process of identification by which objects are recognized as members of particular named categories. The second is primacy in ontogenetic development.

Hunn (1975) reports that the psychological processes involved in classifying flora and fauna as members of some generic category involve holistic, or gestalt, perception while feature identification is required for classification at more subordinate levels. This distinction in identificational routines

holds for people for whom the domain of interest is a significant or highly salient one. In such cases the folk genera are the least inclusive categories to be recognized in a gestalt fashion, and assignment of some stimulus to a generic category occurs prior to identification at the specific or varietal level (with some exceptions for anomalous species). It is at the generic level that categories seem to be maximally distinct.

Bulmer and Tyler (1968) come to much the same conclusions in their investigation of the classification of frogs among the Karam of New Guinea: "We believe that our evidence demonstrates that Karam nonetheless perceive frogs as grouped into 'natural kinds' or 'speciemes' (roughly equivalent to folk genera) marked off from each other by multiple distinctions of appearance, habitat and behavior" (1968:379). And they write further that the "Karam taxonomy is largely based on the people's appreciation of natural species [folk genera]" (1968:350).

Evidence from ontogenetic development is further suggestive of the primacy of the folk genera. Stross (1973), who investigated the acquisition of ethnobotanical nomenclature among Tzeltal Mayan children, found that the typical Tzeltal child begins to acquire botanical terminology by learning the generic names for culturally significant plants. From this starting point the child differentiates and generalizes plants simultaneously as he continues to increase his inventory of generic categories. Stross's research relied upon children's abilities to name about two hundred distinct plant specimens along a standardized plant trail. Diachronic development was inferred from the synchronomic variation in the ability of children of different ages to classify plants. Figure 1 summarizes Stross's results and presents the sequence he posits for the acquisition of ethnobotanical terminology.

By age four the Tzeltal child typically knows more than one hundred botanical terms, the bulk of which are folk genera. The life-form terms are included in his repertoire at this age but are not yet defined appropriately. Even so, some notion of the core concept of each life-form category has developed, and flora of relatively low salience may be recognized as members of a particular life-form category or other suprageneric category before the appropriate generic designation is learned. By age six the child's definition of the life-form categories closely approximates the adult model. Since folk genera are acquired throughout childhood (and adulthood as well), the child continues to learn new generic distinctions subsequent to his mastery of the life-form categories. For the bulk of

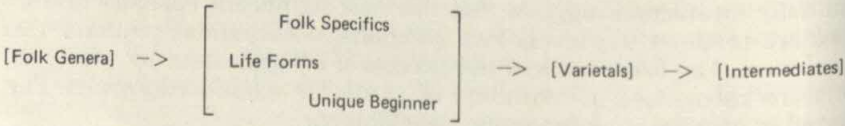

FIGURE 1. The sequence for the acquisition by Tzeltal children of botanical categories at distinct taxonomic ranks. The linear sequence implies that at least one member of any taxonomic rank indicated will be acquired before any categories at a rank on its right.

the plant world, however, particularly for salient members, the first classification learned is at the folk generic level.

In sum, the taxonomic, linguistic, and behavioral evidence from populations maintaining a high degree of functional interaction with their botanical environment point to the folk generic level as the most fundamental and most salient level of the taxonomy. Figure 2 illustrates the convergence of these criteria at the folk generic level.

This view of a single most salient level of classification is a normative one. In general, taking the whole domain into account and all of the various contexts in which botanical classification occurs, we see that the folk genera comprise the most salient distinctions. In the case of highly salient cultigens or marginal plants of little or no significance, the most salient, most usual or frequent, level of classification may vary. We will return to a consideration of this kind of variability in the discussion to follow. For the majority of plants, however, the folk generic classification is more salient than either the superordinate or subordinate categories by which a plant may be classified.

On the basis of the principles derived from the comparative study of classification as summarized above, it might be expected that not only in the specific cases mentioned but in all cases of biological classification, the folk genera are psychologically the most salient category distinctions. However, a number of recent investigations suggest that this is not the case. Rosch and her colleagues (1976) propose (like the ethnoscientists before them) that there is one level of abstraction at which the most basic category cuts for a particular domain are made. In contrast to the ethnoscientists they conclude that this level is superordinate to the folk genera.

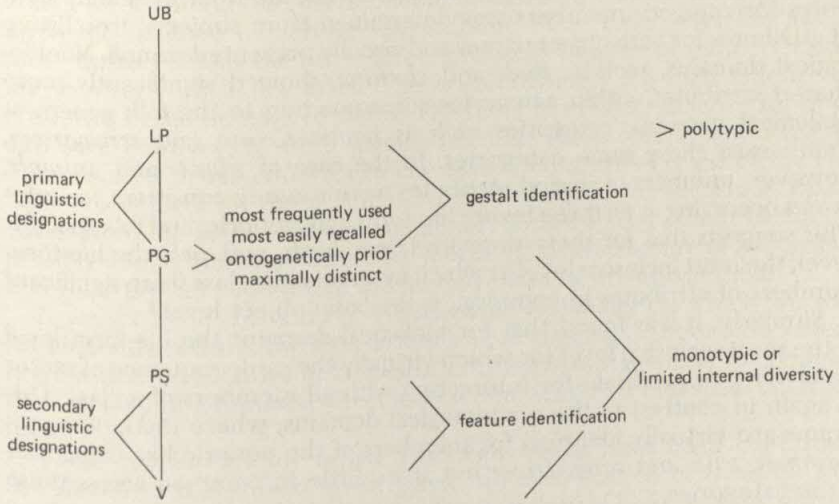

FIGURE 2. The features associated with categories at particular ethnobiological ranks by peoples for whom the biological environment is highly significant. These features determine relative salience of the categories at distinct taxonomic ranks.

They characterize the basic object level categories as the most inclusive categories whose members possess significant numbers of attributes in common, are used by means of similar motor programs, possess shapes that are significantly similar, and can be identified from average shapes of members of the class. These basic categories are further characterized as the most inclusive categories for which a concrete image of the category as a whole can be formed, the first categorizations made during perception of the environment, the earliest categories sorted and the earliest named by children, and the categories that are most codable, most coded, and most necessary in language (Rosch et al. 1976:382). They suggest that these categories best reflect the correlational structure of the world.

Working with Americans living in an urban setting, Rosch and her colleagues applied a series of tasks focused on these dimensions to a number of domains of concrete objects, including some biological domains. Categories at these levels of inclusion, life-form, folk generic, and folk specific, were generally considered in these tasks. In the domain designated by the life-form *trees*, for example, the folk generic categories *maple, birch,* and *oak* and their respective folk specifics *silver* versus *sugar maple, river* versus *white birch* and *white* versus *red oak* were used. Occasionally the unique beginners *plant* and *animal* were involved.

Rosch et al. (1976) hypothesized that there is a basic level of abstraction in classifications of common objects, which is the most inclusive level at which members of a class share a significant number of attributes. That is, members of basic object level categories are expected to share significantly more attributes in common than do members of superordinate categories; while members of categories subordinate to the basic object level should not share significantly more attributes in common than do members of basic level categories. The number of shared attributes for category members was determined from subjects' free listing of attributes for both object names and visually presented stimuli. Nonbiological domains, such as *tools* and *clothing*, showed significantly more shared attributes within categories corresponding to the folk genera of biological domains, categories such as *hammer, saw,* and *screwdriver,* than across these same categories. In the case of *plants* and *animals,* however, numbers of shared attributes were roughly consistent for categories occurring at all three levels: life-form, folk generic, and folk specific. This suggests that for these domains (*trees, birds,* and *fish*) the life-form level, the most inclusive level at which members of a class share significant numbers of attributes in common, is the basic object level.[5]

Similarly, it was found that for biological domains the life-form level is the most inclusive level for which virtually the same sequence of motor behavior is appropriate for interacting with all members of a class. This is again in contrast to the nonbiological domains, where the motor programs are virtually identical for members of the generic like categories *hammer, saw,* and *screwdriver* but share little in common across these same categories.

A naming task yielded corroborative evidence. Subjects were found to name biological stimuli consistently at the life-form level (*tree, bird,*

or *fish*) rather than at subordinate levels (*oak* or *white oak*). Further investigation showed that they not only preferred to name at the life-form level but that in most cases they could not name at any subordinate levels. That is, American subjects could not classify stimuli as members of any particular folk generic or folk specific category.

Investigation of shape similarity among category members was not extended to the botanical domain but was applied in the domain of *animals* including the subgroups *dog, cat, fish,* and *butterfly.*[6] A significantly greater overlap in shape was found for each of the subcategories above than for the superordinate category *animal.* Averaged shapes of members of these subcategories were readily identifiable, while an "averaged animal" was not. These results suggest that *dog, cat, fish,* and *butterfly* are perceptually more homogeneous and therefore, according to Rosch and her colleagues, more basic or salient categories than is the unique beginner. Rosch et al. appear to assume that these four categories are of an equivalent degree of abstraction. However, for at least some speakers of American English this is not the case. *Butterfly* may be a folk generic category immediately included within the life-form *insect,* which is, in turn, immediately included within the unique beginner *animal.*[7] *Dog* and *cat* are either folk genera or intermediate categories immediately included within the life-form designating furry animals variously referred to as *mammal* or *animal* in a restricted sense (Gal 1973). *Fish* (again for at least some speakers of American English) is a life-form category that is immediately included within the unique beginner *animal.*

It is interesting that Rosch et al. (unwittingly?) selected subcategories that fall at different levels of taxonomic inclusion for at least some urban Americans. This suggests that for them as well as for their subjects, basic object level can vary across taxonomic ranks within a domain depending upon the salience of various subgroupings. The basic level for *fish* is more inclusive than the other three, probably because of subjects' minimal contact with living fishes. This is analogous to the high salience of life-forms in the domain of plants. The level of greatest salience for dogs and cats, however, is more specific because these creatures are highly significant for urban Americans. The case for butterflies is less clear. The basic object level for them ranges from folk generic to life-form as *insect* varies from life-form to unique beginner. *Butterfly* is a salient category, possibly due to both the distinctive morphological structure of butterflies and their prevalence among the limited biota of urban America.

If we set aside this kind of intradomain variability (to which we will subsequently return), the data of Rosch et al. suggest that the basic object level within taxonomies of biological entities is suprageneric for populations maintaining minimal direct interaction with their biological environment.

These conclusions for urban American adults have been confirmed by Chambers (n.d.), who investigated the classification of trees by five San Francisco Bay Area residents. Using a variety of tasks (sorting, elicitation of names, identification, and recognition), she finds that informants rely on suprageneric levels of classification. Her informants were able,

for example, to list up to fifty tree names and could recognize over one hundred names as referring to folk generic categories of trees, but their identificational abilities were considerably more limited. Informants were only able to identify particular specimens as members of the appropriate folk generic category for ten to thirty-four distinct folk genera. For the majority of these they could either only recognize prototypical specimens or they overgeneralized the category to include a number of related but distinct species of trees. Chambers found that classification of a specimen at suprageneric levels, either as *needle-bearing, leaf-bearing, frond-bearing*, or just as *tree*, involved gestalt recognition, but that identification at the level of the folk genera typically involved a feature identification routine similar to that proposed for the identification of subgeneric categories by Hunn (1975).

Neither in the work of Chambers nor Rosch et al. is it clear which suprageneric level tends to be basic within the domain of trees: the intermediate opposition of *leaf* to *needle* to *frond-bearing trees* or the life-form category *tree* itself. Chambers's (n.d.) data seem to suggest the former is more significant. She notes, for example, that in the case of one informant, they had essentially only one label for the whole realm of evergreen conifers, and that label was *pine*. Occasionally when a specimen corresponded to an ideal type image, *redwoods* or *junipers* might be correctly identified as such, but even in these cases they remained conceptually *pines*. In most cases, even if a specimen could be correctly identified at the folk generic level, it was more likely to be called *pine*.

Rosch et al. failed to include intermediate level distinctions in their experiments. However, on careful examination of their data, the importance of intermediate level distinctions is apparent. When Rosch and her colleagues asked subjects to list the attributes shared by members of the category *tree*, they elicited a series of attributes the first mentioned of which is specific to *leaf-bearing trees*, that is, leaves. Other attributes, while not specific to *leaf-bearing trees*, were more characteristic of this category of trees than of, for example, *frond-bearing trees*. These attributes include bark, wood, and sap. It is possible that although Rosch et al. were asking for attributes of *trees* in general, they were eliciting attributes of the most commonly encountered intermediate category of *trees*, *leaf-bearing trees*. Since their examples of folk generic categories of *trees—oak, maple*, and *birch*—are all *leaf-bearing trees*, the experimental design itself may have focused informants on this intermediate category. It is, therefore, unclear as to whether the basic object level falls for Americans for the domain of *trees*. It is clear, however, from the results of both Chambers and Rosch et al. that the level is superordinate to the folk genera.

Ontogenetic development provides further evidence that the most salient level of the botanical domain for urban Americans is suprageneric. The classificatory abilities of six American children living in Berkeley, California who ranged in age from three to eight years was investigated by Dougherty (n.d.). Each child participated in a series of naming and identification tasks over a period of months. Diachronic development

was observed over the period of the investigation and was further inferred from the synchronic variation in children's performances. Although the number of children observed was small, the evidence suggested a developmental sequence.

Early in the urban American child's development of classificatory skills in the domain of plants is the acquisition of life-form distinctions and intermediate level distinctions within these categories. During the third and fourth years of age, a Berkeley child segregates the plant world (for which he has no unique beginner designation) into a few major classes. The Berkeley children in this age range distinguished *trees* (tall woody plants) from *plants* (small herbaceous plants) from *cactae* from *bushes* or *grasses*. As the child grows older the number of life-form distinctions gradually increases to about eight or nine.

Within each child's early life-form domains he makes a number of subordinate distinctions. Unlike the Tzeltal child these are not folk generic distinctions but intermediate ones. The distinctions within the domains of *trees* and *bushes* oppose *leaf-bearing* to *needle-bearing* to *frond-bearing* members. The children create such labels as *leaf tree, pine needle tree,* and *pine cone tree* to designate these classes. Within the domain of *plants,* flowering specimens are consistently distinguished from leafy ones.

Like the Tzeltal children, the Berkeley children also learn folk generic names among their first plant designations. Unlike the Tzeltal child, however, relatively few such labels were included among the earliest learned plant names for Berkeley children, and those few generally referred to plant products, such as *banana* or *spinach,* for which the child could not identify the plant of origin. In some cases the child was unaware that the product he could label originally came from a plant. In the case of *spinach,* for example, one child knew only that it was something to eat that could be obtained in a can.

Up to four years of age, Berkeley children consistently distinguished and named plants only at suprageneric levels in marked contrast to the Tzeltal children, who at the same age consistently named at the generic level in addition to and more often than naming at more superordinate levels. The Berkeley children did perceptually distinguish a few anomolous plant specimens and exclude these from their named categories, but they had no linguistic designations for such plants. The exclusion seems to be based entirely on immediate perception rather than learned classification.

As the Berkeley child's knowledge of the botanical world expanded and his classificatory system continued to develop, classification on the intermediate level proliferated. Children subdivided the life-form domains in terms of numerous intermediate level distinctions: members with leaves, needles, berries, flowers, or thorns. Distinctive but often temporary or seasonal traits formed the basis for this intermediate classification. During this same stage (approximately five to six years of age) the Berkeley children learned their first consistent folk generic distinctions. The plants initially designated at this level were typically found

in the family garden or were species commonly used for ornamentation or food, that is, highly salient members of the plant world. *Rose bushes, blackberry bushes, fuschia,* and *western cactus* are examples.

Gradually the Berkeley child's dependence upon temporary characteristics diminishes and the life-forms are resubdivided on the basis of more permanent morphological features but still at an intermediate level. Folk generic distinctions continue to be acquired and by eight the Berkeley child typically includes about a dozen folk generic distinctions such as *eucalyptus, redwood,* and *maple* in his repertoire. By eight years of age the child has also acquired his first specific designations. *Domestic* versus *wild strawberries* is one example.

From seven or eight on, the number of perceptual/conceptual distinctions that the Berkeley child can make consistently is increasing. Many of these he may never learn to label. One child, for example, consistently recognized *atlas cedar* and *canary island pine* as distinct kinds of *trees* although he could not linguistically differentiate them. Simultaneously, and somewhat paradoxically, as the child's ability perceptually and conceptually to distinguish plant specimens outstrips his ability to name these distinctions, he begins to acquire a repertoire of folk generic plant names, such as *avocado, begonia* and *hens and chickens,* that he may never learn to associate with any actual referent.

Figure 3 presents the general acquisitional sequence observed for the Berkeley children's development of ethnobotanical classification.[8] In terms of both the number of distinctions acquired and the specificity to which classifications can be extended, the eight-year-old Berkeley child lags well behind the six-year-old Tzeltal child. As Chambers's (n.d.) evidence shows the ethnobotanical knowledge of urban American adults may develop minimally beyond that of the eight-year-old child.

Work by Rosch et al. (1976) provides further data on ontogenetic development and shows that basic level categories are acquired before superordinate distinctions. They found that the categories *dog, cat, butterfly,* and *fish* are acquired prior to the more inclusive category *animal.* In a series of triad sorts given to children three to seven years of age, they found that performance was virtually perfect for all children at the basic level, but that superordinate sorts (sorts that depended upon mastery of the category *animal*) improved for children from three to four years of age. That is, basic level sorts were equally easy for all age groups, while mastery of the superordinate level was not achieved until four years of age. Unfortunately, categories subordinate to the basic level were not included in these experiments.

The evidence presented from the investigations of biological classifica-

FIGURE 3. The sequence for the acquisition by urban American children of categories at distinct taxonomic ranks. The linear sequence implies that at least one member of any taxonomic rank indicated will be acquired before any categories at a rank on its right.

tion among urban Americans suggests that categories superordinate to the folk genera are typically more salient in biological domains than are the folk genera themselves. As Figure 4 shows, the greater salience of life-form and intermediate categories over all subordinate distinctions is reflected in frequency of use, ease of recall, ontogenetic priority, and gestalt identification. Informants find these categories to be well differentiated from one another and to contain little internal diversity. In classifying, particularly the botanical environment, few distinctions of greater specificity than the intermediate categories are made. Although these categories may be known to contain distinguishable members, these members are not habitually distinguished and, for most informants, are not identifiable.

RELATIVITY OF BASIC OBJECT LEVEL

While the number of societies sampled above is limited, the generalization is likely to prove correct that the most salient or fundamental categories in biological classification are not fixed by nature but vary as a function of an individual's or a culture's degree of interaction with the domain concerned. Minimal significance of some segment of the environment is reflected in the decay of the classificatory system. Urban Americans are ignorant of the attributes that characterize groupings of organisms subordinate to the major life-form classes. Americans involved in the experiments of Rosch et al. were able to provide few attributes character-

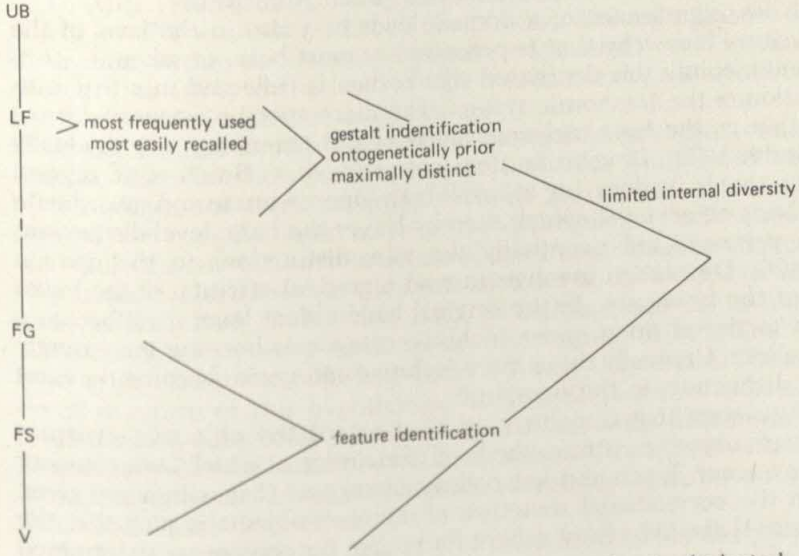

FIGURE 4. The features associated with categories at particular ethnobiological ranks by peoples for whom the biological environment is of minimal significance, requiring minimal direct interaction. These features suggest the relative salience of categories at distinct taxonomic ranks.

istic of subordinate groupings of trees that were not also generally characteristic of all trees. Populations interacting consistently with their botanical environment would likely expand the list of attributes associated with particular folk generic and folk specific categories of trees by adding features characteristic of the bark, wood, sap, foliage, or habitat. The motor programs associated with groupings of organisms are also related to the significance of those organisms for some population. In contrast to the passive motor routines that characterized Americans' interactions with trees (Rosch et al. 1976), specific motor programs for planting, harvesting or felling, extracting sap, or stripping bark might be represented among the motor routines associated with distinct categories of trees by nonurban peoples.[9]

Chambers (n.d.) has shown that urban Americans are unable to identify and name distinct folk generic categories of *trees*. Berlin (1972:83) claims many contemporary urban Americans know virtually no specific names for kinds of plants and may recognize folk generic names only as belonging to the appropriate domain without being able to associate them with any referent. He suggests that only the more inclusive life-form names such as *tree* or *grass* are useful terms for urban Americans referring to the botanical portion of their natural environment.

A review of extensive speech samples collected from a young urban American child during the early stages of her linguistic development (Rosch et al. 1976; Brown 1974) shows that she makes no reference to any members of the botanical domain. At the same time she does refer to some members of the animal kingdom. This again suggests minimal significance of the botanical domain for urban Americans.

Such low significance of a domain leads to a rise in the level of the classificatory hierarchy that is perceived as most basic or salient.

Simultaneously this decreased significance is reflected in attrition or devolution of the taxonomic system. The more specific category distinctions, that is, the least inclusive levels of the taxonomy, are gradually lost (Berlin 1972). Devolution does not progress as Rosch et al. suggest it might, that is, by decaying at levels both superordinate and subordinate to the basic object level, which thereby leaves the basic level distinctions as the primary (and eventually the sole) distinctions in the domain (1976:426). Devolution involves instead a gradual attrition of the lower levels of the taxonomy. As the original basic object level itself becomes subject to this attrition, more inclusive categories become increasingly more salient. Gradually these more inclusive categories become the most salient distinctions in the domain.[10]

The question that remains is to what extent the objective structure of concrete objects constrains the level of inclusion at which basic category cuts may occur. Rosch and her colleagues suggest that within any given domain the correlational structure of member objects is such that the categories that will be most salient for human perceivers are determined by the objective structure "out there." Typically, they suppose, within any one domain, there is only one potential level that will be perceived by human observers as basic. Some domains, they suggest, are structured

such that two levels are potentially basic. In these cases cultural focus will influence which of the two levels will be perceived as basic. From the perspective of Rosch et al., factors such as individual expertise and familiarity may also influence the relative salience of category distinctions, but this is only supplementary to the naturally occuring objective distinctions among objects. For instance, the animal domain, according to Rosch et al., permits of two potential basic object levels. *Bird* may be basic for some, while *gull, chicken,* or *turkey* may be basic for others (Rosch et al. 1975).

There is no reason to stop here. Expertise and indifference are not atypical states for human beings with respect to particular domains. Their effects on classification are consistently significant factors determining which object classes are most salient. The limits on the degree of specificity or generality at which basic category cuts can occur must be considerably wider than has been postulated. Among a people for whom distinct kinds of trees are generally of minor importance, the most salient contrast in their classification of the botanical domain might fall at the life-form level where *tree, vine,* and *bush* contrast, or at an intermediate level where *leaf-bearing, needle-bearing* and *frond-bearing trees* contrast. For the denizen of a future colony in outer space the contrast of *plant* and *animal* might well be more salient than any subordinate categories. Among people for whom the distinctions among tree genera are consistently significant the most salient category cuts will tend to be folk generic. And more specific distinctions may be the most salient categories for highly significant members of the domain.

To what extent, then, is objective structure a determinant of basic object level? The answer is not an obvious one. The most salient category distinctions within the domain of *furniture* are established for Americans (Rosch et al. 1976) at the level at which *table, lamp,* and *chair* contrast because at this level the categories are maximally distinct in terms of their correlational structure while their internal membership is minimally diverse. Consider, however, a hypothetical society (modeled on some of the indigenous Oceanic groups) where the domain of *furniture* in general is of roughly equal salience to the people as it is for Americans, but where the correlational structure of the domain membership is markedly distinct. Here furniture is made of bamboo or wood, but whether it is a *pillow, stool, chair, table, shelf,* or *bed* its construction and shape is the same. All pieces of furniture are rectangular objects. The distinct types of *furniture* (each of which is named) vary only in size. The correlational structure of this hypothetical domain of *furniture* indicates that level at which categories are maximally distinct while their membership is minimally diverse occurs in the contrasting of *furniture* with *tools.* It is not at all obvious, however, that these categories would be more salient than subordinate distinctions.

While the correlational structure of physical objects must play a role in determining at what level of inclusion basic category cuts will fall, the primary variable in determining relative salience in domains of concrete objects is more likely the relationship of the human classifier to

the members of the domain. Those classes that are seen as best reflecting objective structures vary according to the interests and attention of human groups and individuals.

NOTES

1. Many thanks to Brent Berlin, Paul Kay, Eleanor Rosch, Carolyn Mervis, Roger Brown, John Gatewood, and Midge Solberg for their criticism and discussion.
   An adequate characterization of the cognitive processes involved in biological classification must account for the conjunction of taxonomy with other systems of cognitive organization including prototypicality, overlapping, and ambiguous category membership (Brown 1976; Randall 1976; Rosch et al. 1976), and chains of categories related along a similarity continuum (Hunn 1973, 1976). The relative importance of the various modes of cognitive organization may vary with the nature of the domain concerned and with particular cultural and individual tendencies. However, in the classification of the biological world, contrast and inclusion and multilevel hierarchies are consistently salient features of man's classificatory behavior.
2. Although intermediate distinctions have been observed to fall primarily between the life-form and folk generic categories, they may occur between any two of the five major ethnobiological ranks. The folk genus *oak*, for example, is partitioned by the intermediate categories *red oak* and *white oak* by many Illinois residents. Each of these intermediate distinctions includes a number of specific categories of *oak*. *White oak* includes, for example, true *white oak*, *burr oak*, and *post oak*. Among the folk specifics included in *red oak* are the true *red oak*, *black oak*, and *pin oak*.
3. The picture for the naming of ethnobotanical folk categories is not quite as simple as portrayed here. Within any given level (particularly among the folk genera and folk specifics) the nature of a category's linguistic designation is correlated with its cultural significance (Berlin, Breedlove, and Raven 1974:96–99). The greater the cultural significance of a given category, the most likely it is to be designated by a primary rather than a secondary lexeme or by a simple primary rather than a complex primary lexeme. The more salient a category is (within any particular rank), the more likely it is to be designated by a relatively brief, semantically opaque linguistic label.
4. I wish to thank John Gatewood for suggesting this point.
5. Unfortunately, categories superordinate to the life-forms, that is, the unique beginners *plant* and *animal* were not incorporated into Rosch's experiments even after it became apparent that the basic object level might be of greater abstraction than originally hypothesized.
6. It would be interesting to see just how similar are the normalized shapes of distinctly different trees, such as *maple*, *fir*, and *palm*. The overlap in shape for category members within the domain of *trees* may not be as great as Rosch et al. assumed on the basis of the three folk genera *oak*, *maple*, and *birch*.
7. A survey of twenty undergraduate and graduate students at the University of Illinois shows that for about half of this group *butterflies* are kinds of *insects* that are, in turn, kinds of *animals*. For the other half of this group, however, *butterflies* are kinds of *insects*, but *insects* are not included within the domain of *animals*. *Insects* for these informants is a unique beginner contrasting with *animals*, and *butterfly* is a major life-form category within it.
8. Cognitive and linguistic categories are not consistently distinguished in the acquisition studies for Tzeltal (Stross 1973) and Berkeley children. These are

more clearly differentiated in the supplementary study by Rosch et al. (1976) described below, but further research is required in this area.

9. The role of cultural significance in determining basic object level may offer at least a partial explanation for the greater specificity found for nonbiological as opposed to biological domains for urban Americans (Rosch et al. 1976). The nonbiological domains selected by Rosch et al. (1976) for investigation, such as *tools, clothing, furniture,* and *vehicles,* are of high significance to Americans. Correspondingly the most salient level in these domains is less inclusive than in marginally significant biological domains. The basic object level in these nonbiological domains falls not at the level at which *tools, clothing,* and *furniture* contrast (analogous to the life-forms of biological domains), but at the level at which *saw, screw-driver,* and *hammer* or *pants, socks* and *shirts* contrast (analogous to the biological folk genera).
10. Categories are encoded in a similar fashion. The basic objects are the first to be linguistically encoded. Since those categories that will be basic depend upon cultural emphases, the categories that will be first encoded for any particular domain may vary. It has been suggested that folk genera were the first categories linguistically encoded by man (Berlin 1972) because of the high significance of natural species to early human groups. However, life-form distinctions appear to be the first encoded and most numerous distinctions in American sign language (Rosch et al. 1976)—due, I suggest, to the marginal significance of natural species for users of American sign language.

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III
Extended and Transferred Meaning

In recent years, the semantic analysis of folk classification systems has been advanced in several directions. Through componential analysis, as was shown in Section II, it is possible to specify the criterial, or distinctive, features that determine the categorization of referent entities (denotation), differentiate the semantic categories in classification systems (designation), and define the referential meaning of each of the lexical items in the classification system (signification). One area in which our understanding of referential semantics has progressed is in the study of the extension of referential meaning, most particularly in the analysis of kinship terminologies, and recently in studies of other semantic domains as well. Another area of advance is the study of figurative, or transferred, referential meaning, most notably in analyses of metaphoric transfer. This section will be devoted to the analysis of both extended and transferred meaning.

EXTENDED MEANING

The first chapter in this section, Chapter 9, is concerned with both of these areas of referential semantics, as well as several others, and can serve as an overall guide to this section of readings. Although Susan S. Bean analyzes the meaning of only a single lexeme, *amma* in Kannada, she elegantly elucidates the systematic relationships among the multiple meanings of the term: "mother," "woman," "goddess," "pox," and "help!" Her discussion of these senses of *amma* not only illustrates the types of referential meaning analyzed in the chapters in this section, but also provides a framework for interrelating these separate analyses.

Studies of the extension of referential meaning assume that all the referent entities denoted by a lexeme are not equivalent, that one (or several) of them is primary and that others are secondary. It could be argued, and in fact is argued by some anthropologists, that no distinctions can be made among the referents of a term, as they are all members of a single category. The view that lexemes are not generally monosemous, but rather polysemous—that is, that they have a primary sense and one or more secondary senses—has been pioneered and convincingly expounded by Floyd G. Lounsbury (1964, 1965, 1969) and by Harold W. Scheffler and Lounsbury (1971). In studies of kinship

terminologies, it has been shown by Lounsbury and many others that the primary referential meaning of a kin term derives from the genealogically closest of the types of kin relationships (kintypes) that are included among the referents of the term. This primary, or focal, sense of a kin term, defined by a set of distinctive features, is extended to the more distant referents, which represent the secondary, or extended, senses of the term. Extension of meaning from focal to extended referents involves a widening and weakening of that meaning—features that are criterial in the definition of the focal sense of the term are neutralized; that is, they are noncriterial in the definitions of extended senses. Analyses that assume that kin terms are monosemous—that the referents included in kin categories cannot be differentiated—treat the broadest, most attenuated senses of the terms as their only meaning and, as a result, account only partially and very generally for the referential meaning of the terms.

We can take up Bean's discussion of Kannada *amma* to illustrate meaning extension. The primary referent of *amma* is mother, but it also denotes father's mother and mother's mother, mother's elder sister and father's elder brother's wife, and husband's mother. The primary, or focal, meaning of *amma* (mother) is defined as "kinsman, lineally related, in the first ascending generation, and female." Bean uses a different notation system, but we can represent this focal meaning as +KINSMAN, +LINEAL, 1ASCENDING GEN., and −MALE. As Bean demonstrates, the extended meaning conveyed by *amma* in reference to father's mother and mother's mother is wider than the focal meaning of the term, in that the contrast between first and second (and probably all other) ascending generations is neutralized, and the extended sense signified in reference to mother's elder sister and father's elder brother's wife involves (among other things) the neutralization of the +LINEAL (linked by only lineal—i.e., parent-child—relations) versus −LINEAL (linked by a collateral—i.e., sibling—relation) contrast, but not the contrast between first and other ascending generations. A point to note here is that focal senses may be extended along several lines and that extended senses may be partially overlapping.

Category Grading

It will be recalled that primary, focal categories and secondary, more inclusive categories with indefinite boundaries were encountered previously in studies of color classification systems (Chapters 6 and 7). It was shown in this research that although informants are very consistent in locating the primary referents of color categories (both within and across languages and even for composite or macro-categories with two foci), they vary considerably in the secondary referents they include as members of these categories. In Chapter 10, Willett Kempton takes up the issue of fuzzy boundaries in his discussion of *category grading*. As shown in Section II, the standard definitions of the inclusion and contrast relations treat categories as internally homogeneous and sharply bounded, and do not allow for gradations in category membership or in relations among categories. Kempton's aim is to demonstrate that objects classified in categories in the limited domain "containers for liquids" differ in amount of category membership and that contrasting categories in the domain differ in the degree to which they are included in superordinate categories. In an analysis of a portion of this domain, he uses "fuzzy set theory" to account for both fuzzy category membership and fuzzy inclusion relations.

Fuzzy set theory makes it possible to account for the fact that objects are members of categories to a greater or lesser degree. Whereas in standard taxonomic theory categorization is discrete, with each object being classed as either a category member or not a category member, in fuzzy set theory categorization is graded, each object being characterized by the degree to which it is or is not a category member. In this way, intermediate grades of membership are recognized and central members of categories are distinguished from peripheral members. The core, or primary, referential meaning of the category derives from the central referent, and this core sense is extended to decreasingly similar peripheral referents. A coffee cup, for instance, is a central member of the *cup* category, whereas a paper cup is a peripheral member.

Kempton also significantly revises fuzzy set theory so that it can be used not only in analyzing the grading of category membership, but additionally in analyzing the grading of relations between categories. Just as objects are members of categories to a degree, so also categories are included in superordinate categories to a degree. As Kempton neatly shows, the category *coffee cup* is included to a greater degree in the immediately dominating category *cup* than is the category *mug,* which is also a kind of *cup.* The relation between *coffee cup* and *cup* is one of *central inclusion* whereas the relation between *mug* and *cup* is one of *peripheral inclusion.* The important point to note here is that Kempton's adaptation of fuzzy set theory to include fuzzy category relations provides a basis for analyzing the extension of referential meaning in domains other than kinship. Analysis of equivalence relations between the focal and extended senses designated by kin terms is facilitated by the existence of genealogically-defined kin types, which provide a means of analyzing the membership of kinship categories and of distinguishing central and peripheral members. With the approach described by Kempton, relations between focal and extended senses can be analyzed in cases where no ready means for examining the relations among referents is available. Recently, fuzzy set theory has been used in an important analysis of color classification (Kay and McDaniel 1978), and it can be expected to be used in advancing the study of other types of classification systems in the near future.

TRANSFERRED MEANING

The other area of referential semantics mentioned at the beginning of this section as having seen progress recently is the study of figurative, or transferred, referential meaning. Bean, in her analysis of the senses of *amma,* contrasts two fundamental processes of semantic transfer: *metaphoric* and *metonymic.* Metaphoric transfers occur on the basis of similarity relations between the primary (or extended) referent of a term and a second referent to which the term is transferred. Metaphoric transfer is achieved by neutralizing one or several of the distinctive features of the primary sense of the term and by treating one or several of the connotative features associated with the primary sense as criterial. For example, when it is said of a person that he or she is a cold person, a metaphorical meaning is achieved by neutralizing the features of physical state that define the literal, or primary, sense of *cold* (lacking warmth or heat energy) and by treating connotative features of the primary sense of *cold* (stiff, unmoving, rigid) as distinctive features of the metaphorical sense. The neutralized features are

not completely lost but rather are carried along as nondistinctive connotative features of the metaphorical sense of the term. Thus, when it is said that so-and-so is a cold person, the metaphorical implication is that he or she is emotionally stiff and unmoving, as if physically lacking heat.

Metonymic Transfer

Metonymic transfers occur on the basis of contiguity relations between the primary referent of a term and a second referent to which the term is transferred. Transfer is not based on a similarity between the two referents but instead on a contextual association between them—that is, on the fact that they usually or generally occur together in specifiable contexts. Because similarity is not the basis of transfer, metonymic senses are not achieved through changes in distinctive and nondistinctive features. Rather, all the distinctive features of the primary sense of the term are neutralized (and transferred only as connotative features), and features of the referent to which the term is transferred become distinctive. We have seen that the English word *cold* in its literal sense refers to the physical state of lacking heat and that in its metaphorical sense it refers to a human personality trait. It also has a metonymic sense. In saying of someone that he or she "caught a cold," a metonymic meaning is conveyed by neutralizing the physical-state features of *cold* and substituting features that define a respiratory illness that is believed to be contextually associated with the physical state. Bean's discussion of the "pox" sense of *amma* provides further illustration of metonymic transfer.

Metaphoric Transfer

Few studies have yet been attempted of metonymic transfer in folk classification systems, but recently considerable attention has been focused on metaphoric transfer. The final two articles in this section illustrate this direction of research. Bean's analysis of the metaphoric transfer of *amma* from the primary sense 'mother' to 'woman, politely' can serve as a model of the analysis of metaphoric transfer. As described earlier, the primary sense of *amma* is defined by the features +KINSMAN, +LINEAL, 1ASCENDING GEN., and −MALE. In addition, as Bean points out, *amma* has several associated connotations that are basic to its transferred metaphorical senses. One of these connotations associated with *amma* refers to the status of mothers in society. Females in Kannada-speaking areas of India gain positions of respect and authority as mothers. As an unmarried girl, a wife, and a daughter-in-law, a woman has inferior status and owes deference, but as a mother she achieves superior status and is owed deference. So one meaning that is connoted by *amma* is "status entitled to respect."

As Bean shows in her chapter, this connotative feature of the primary sense of *amma* is a distinctive feature of the metaphorical sense of the term. Because most women are mothers and they derive their status in society from motherhood, the categories "mother" and "adult woman" are considered very similar. In the metaphoric transfer of *amma* from primary (and extended) mothers to adult women in general, the features +KINSMAN, +LINEAL, and 1ASCENDING GEN. are all neutralized, only +ADULT (a remnant of these neutralized features) and −MALE are transferred, and the connotation "entitled to respect" becomes

a distinctive feature. The metaphorical meaning of *amma* "woman, politely" is therefore defined by the features +ADULT, −MALE, and +POLITE.

Metaphorical Kin Term Usage

In Chapter 11, my article on the face-to-face use of kinship terms in a Turkish village, an analysis of the metaphorical meaning communicated in the use of a full kin terminological system is presented. It is shown that the metaphorical meanings communicated by kin terms in addressing persons in the village who are not kin are derived from the literal meanings conveyed by the terms in addressing kin. Younger villagers, for example, use an "uncle" term in addressing unrelated elders; the meaning they convey in this usage is a metaphoric transfer from the literal meaning of the term.

Metaphoric transfer is patterned and systematic in the terminological set: distinctive features on four genealogically-defined dimensions are neutralized and transferred only as connotative features of metaphorical meanings; distinctive features on two dimensions are carried over as distinctive features of metaphorical meanings; and connotative features on two dimensions associated with the literal genealogical meanings of the terms are transferred as distinctive features of metaphorical meanings. The distinctive features that are carried over are +MALE (male)/−MALE (female) and 0GEN. (same generation)/+1GEN. (parental generation)/+2GEN. (grandparental generation), etc., and the connotative features that become distinctive features are →POWER (status superior to Ego, entitled to respect)/←POWER (status inferior to Ego, not entitled to respect) and →SOLIDARITY (intimate or cooperative relationship between Ego and alter)/←SOLIDARITY (nonintimate or conflictive relationship between Ego and alter). The power and solidarity dimensions, which are probably universal dimensions of connotative meaning (cf. White 1980), are shown to account both for the pattern of connotative meaning associated with the literal genealogical senses of the kin terms and for the systematic semantic transfer in which these connotative features become distinctive features of the metaphorical meanings of the terms.

Bean makes the point that, in its transferred metaphorical sense "woman politely," *amma* functions as a referential-indexical sign, both referring to a social category and indexing the hearer as the referent of the term in a particular situation of usage (recall the discussion of signs in Section I). In the analysis of the Turkish terminology, it is shown that the terms are used to accomplish both referential and indexical functions: in address to kinsmen, they refer to particular kin categories and index hearers in particular situations of usage. It is additionally shown that in address to nonkin the terms function as referential-indexes, referring to kin categories and indexing referents in situations of usage, and as nonreferential or pure indexes, indicating relative power and/or relative solidarity relationships between speakers and hearers. These separate functions are represented in componential definitions of the nonkin address terms by features on different dimensions: referential-indexical functions are specified by features on the sex-of-relative and relative-generation dimensions, and the pure indexical functions are specified by features on the power and solidarity dimensions.

In addition to transferred metaphorical meaning, Chapter 11 is also concerned with another type of metaphorical meaning, "indirect metaphorical meaning."

In address to nonkin, the kin terms are used both to communicate direct metaphorical meanings, as already described here, and to convey indirect, or switched, metaphorical meanings. In both types of usage, speakers use language to accomplish certain social acts. These are "speech acts," acts that describe or represent how something is in the world (e.g., acts of stating, asserting, claiming, declaring, judging, complimenting, and blaming) and acts that change the way something is in the world (e.g., acts of requesting, commanding, suggesting, promising, legitimizing, and stipulating; cf. Searle 1976, Fraser 1974). Nonkin address usages are of the former representative type. In direct nonkin usages, speakers perform evaluative representative speech acts—acts of identifying or classifying hearers in appropriate semantic categories, of representing the social world and evaluating the hearer's place in it. In indirect nonkin usages, speakers perform attitudinal representative speech acts by way of performing false evaluative acts of classifying. Categorically false acts of classifying cause shifts in the features that define the transferred metaphorical senses of the terms, and thereby imply increases or decreases in relative power, relative solidarity, and relative generation. In this way, speakers indirectly perform attitudinal speech acts—acts of flattering, praising and honoring or denigrating, insulting and degrading hearers; acts that represent the hearer's place in the social world and express an attitude toward it.

'Wise Words' Metaphors

Keith H. Basso's analysis in Chapter 13 of Western Apache "wise words" is also concerned with transfers of semantic features that occur in the formation of metaphorical concepts. "Wise words" comprise a speech genre used by Western Apache to express mild criticism indirectly. "Ravens are widows," "Butterflies are girls," and "Carrion beetle is a white man" are examples. The metaphorical meaning conveyed by "wise words," as Basso shows, depends on the recognition that the literal meaning of these expressions is contradictory or false—carrion beetles are not the same thing as white men—and that a metaphorical meaning based on a presumed similarity between the two main semantic categories in the expression is instead being indicated: Carrion beetles are *like* white men.

"Wise words" express metaphorical meanings by placing in juxtaposition two semantic categories that have both common features of referential meaning and features that distinguish them. This is the same process already encountered in our discussion of the metaphorical senses of *amma* and the Turkish kin terms. As shown, metaphorical meanings are based on similarity relations: adult women are *like* mothers, and nonkin co-villagers are *like* kinsmen, although in fact they are not the same thing. Metaphorical meanings in both cases involve designative features that are distinctive for both semantic categories and connotative features that become distinctive features of the metaphorical meanings. The metaphorical meanings expressed by Western Apache "wise words" are constructed in the same way. Basso very lucidly describes, for example, how the metaphorical sense conveyed by "Carrion beetle is a whiteman" is formed through the transfer of shared features of designative meaning—both "carrion beetle" and "whiteman" are distinguished as living things and as earth-dwellers—and of connotative meaning—both are thought to waste food. The metaphorical concept expressed by "Carrion beetle is a whiteman" is defined by the distinctive features +LIVING THING and +EARTH-DWELLER, which are carried-over

designative features, and +FOOD WASTER, which is the crucial connotative feature in the semantic transfer.

The metaphorical concepts communicated by "wise words" are novel semantic categories that are not lexicalized in the Apache language; they are wholly new, unique concepts created out of familiar semantic materials. This is not to say, of course, that metaphorical concepts do not become frozen; many, in fact, do become fixed and are used as set idiomatic units. Both novel and frozen "wise words," however, are formulated and used in accord with specific sociolinguistic principles and in order to accomplish certain speech acts. The two semantic categories in "wise word" expressions are always similar with respect to negative behavioral attributes—for example, "Carrion beetle is like a whiteman because they both waste food"—and the comparison always serves as a very subtle means of performing indirect attitudinal speech acts: although the person or persons at whom "wise words" are aimed must be inferred from complex background and contextual information, they are always aimed at accomplishing acts of criticizing, suggesting, advising, and the like.

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9

Referential and Indexical Meanings of *amma* in Kannada: Mother, Woman, Goddess, Pox, and Help![1]

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Most studies in ethnographic semantics focus on the structure of readily isolable lexical sets with the goal of delimiting a semantic domain, defining the categories included in it, and describing the system of relationships among them. Aware of the richness of the meanings of words, ethnographic semanticists have recognized that separating definitive from connotative meanings and basic from figurative senses are necessary steps in analysis. Unfortunately, however, most ethnographic semantic analyses have stopped with the definitive meanings of terms.

Connotations and figurative senses of words, building as they do on culturally posited similarities and contextual associations, are as interesting from the point of view of the ethnographer as the literal senses of words. Analyzing figurative senses requires careful consideration of the relationships among cultural categories and therefore contributes to our understanding of a sociocultural system.

In addition to the connotations and figurative senses of terms which are, strictly speaking, referential (see below), some terms have nonreferential meanings. Deictics and expletives, for example, do not signify properties of a designated category alone, but, in the first case, indicate an existential relationship between an object and the situation of utterance, and, in the second case, manifest the state of mind or attitude of the speaker. Although these kinds of senses have been largely ignored, their inclusion promises a further enrichment to the ethnographic study of meaning (e.g., Jakobson 1957; Tambiah 1968).

In presenting an analysis of the meanings of *amma*, literally 'mother' in Kannada, I hope to show that the techniques of ethnographic semantics can take us beyond definitive meanings to the analysis of connotations, figurative usages, and nonreferential senses, and that the procedure is of value both to the analysis of linguistic meanings and sociocultural systems.

THE DATA

In the Kannada of Avaruuru,[2] the term /amma/ (literally 'mother') occurs with several distinct senses. I will argue that these several senses constitute

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[2] Kannada is a Dravidian language spoken by about twenty million people, mostly in the state of Karnataka in India. The data used in this analysis were collected in Avaruuru, a non-Brahmin agricultural village near the city of Bangalore in 1970 and 1971.

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a case of polysemy in which a linguistic form has several distinct but related senses (e.g., *pig* the animal and *pig* a policeman in English), rather than a case of homonomy in which a linguistic form has several distinct and unrelated senses (such as *pair, pare,* and *pear* in English).

As a kinship term, *amma* is used to signify a genealogical relationship, to refer to an individual, or to address an individual:

1. *amma* is used as a term for classifying genealogical relationships; /avaru namage amma aagabeeku/ 'she (plural/polite) to-us mother ought-to-occur,' that is, 'she is related to us as mother.'
2. *amma* is used as a term for referring to an individual; /nam amma hooglilla/ 'our mother did-not-go.'
3. *amma* is used as a term for addressing an individual; /amma baa/ 'mother (or grandmother, father's elder brother's wife, husband's mother) come.'

In addition, *amma* may be used in polite address or reference to a woman, a goddess, a little girl, and occasionally endearingly to an infant boy or angrily to a young boy. That the latter is not a case of homonomy was made clear on one occasion when a mother called tauntingly to her young son: /baamma baaree/ 'come *amma,* come-female vocative suffix'; here, two forms literally for female addressees occur.

In address, *amma* usually occurs with a singular imperative verb:

4. /baamma/ 'come woman, politely.'

In reference, *amma* occurs with a demonstrative adjective:

5. /aayamma bandLu/ 'that woman (politely) came' (*aa* 'that').
6. /iiyamma bandLu/ 'this woman (politely) came' (*ii* 'this').[3]

Further, *amma* occurs in the personal names of women. Personal names usually consist of two parts: a god name followed by a kinship term (*amma* M, *appa* F, *akka* eZ, *aNNa* eB) or one of several suffixes. A kinship term is almost always included if the individual referred to is a non-Harijan[4] adult; it is seldom included if he or she is a child. For example:

7. *lakšmamma*—the name of the goddess of wealth, *Lakshmi,* plus *amma.* A little girl is called *lakšmi.*
8. *muniyellamma*—part of the name of the village god *munešvara* plus the name of a goddess *yellamma* associated with Harijan castes in the area. (Indian goddesses are almost always addressed and referred to with the kinship term attached to the name.) A little girl will be called *muniyelli.*

Finally, *amma* occurs in two senses quite different from each other and from those given so far. In the first, *amma* is used to refer to diseases which cause sores, especially smallpox, but also chicken pox:

[3] These forms are also used ironically or sarcastically to express a lack of deference.

[4] Non-Harijan, or "Children of God," was Gandhi's term for the members of untouchable castes.

9. /nam maguge amma bantu/ 'our baby-to *amma* came (third person, neuter, singular),' or 'our baby has pox.'

This is clearly not a case of homonymy, since the other Kannada term for mother /taayi/ is similarly used to refer to pox.

In the second sense, *amma* is a cry of distress or surprise addressed to no one in particular:

10. */ammaa/ ammaa/* 'help! help!,' 'oh dear, oh dear.'[5]

Grammatical context and intonation are usually sufficient to distinguish the sense of *amma* intended:

> When *amma* is used to refer to a genealogical relationship, it occurs with the idiomatic expression *aagabeeku* 'is related as' (1). When *amma* is used to refer to a kinsman, it occurs with a possessive pronoun (2). When *amma* is used to address a kinsman, it may occur alone or with vocative affixes (/d-, -oo/ i.e., *damma ammoo*) (3). When *amma* is used to denote a woman politely, it may be part of a personal name (7, 8) or occur with the demonstrative adjectives (5, 6). When *amma* is used to designate 'pox,' it occurs with a third person neuter verb (9). When *amma* is used as a cry of distress, the final vowel is lengthened and accompanied by rising pitch (10).

In some cases, knowledge of the social context is necessary in order to decide which sense of *amma* is intended. For example, /baamma/ 'come *amma*' can be used to address mother or a woman politely (2, 3).

CONCEPTS

We are confronted with four distinct classes of senses whose relationships to each other are not immediately obvious. In the first, *amma* is used to designate kinsmen: mother, and may be extended to grandmother, mother's elder sister, and husband's mother. In the second, *amma* is used to politely address or refer to a woman, a girl, or a goddess. In the third, *amma* stands for the disease category 'pox.' And, in the fourth, *amma* is a cry of distress or surprise. The several senses of *amma* are related to each other, and the relationships can be discovered by analyzing the uses of the term in the context of beliefs about women, the statuses they occupy, and the roles they play.

In order to proceed with the analysis of *amma*, we distinguish two functions of linguistic signs: referential and indexical. The sign functions discussed here are taken from Peirce's trichotomy of signs: symbolic, indexical, and iconic (see Burks 1949; also Jakobson 1957; Peirce 1940). An *indexical sign* indicates its object through an existential relationship with it; an *iconic sign* represents its object by similarity to it; and a *symbolic sign* refers to its object through an arbitrary relationship established by convention. Each of these sign functions is characterized by the relationship between the sign and its object. A sign functioning referentially

[5] The glosses are inadequate; the reason for the inadequacy will become clear in the "Analysis" section.

designates a concept or object in the sociocultural universe. A sign functioning indexically indicates a relationship in physical, temporal, or social space between such an object and the situation of its utterance. For example, the pronoun 'you' is indexical because it is actually directed at its object (the object of 'you' is the actual recipient of 'you'); the expletive 'gosh!' is indexical because it is in existential relationship with, a manifestation of, its object, some inner state of the speaker. Linguistic signs functioning indexically must be carefully distinguished from those functioning only referentially. The meanings of indexical signs are pragmatic, related to the user and the situation of use. Referential signs, on the other hand, denote things in the sociocultural universe without relating them to the situation of utterance. Any particular sign can, and often does, combine more than one of these sign functions. Thus the pronoun 'you' both refers to and indicates the addressee.

We may distinguish several types of referential senses according to the relationships among the designata of the polysemous term. The primary or literal sense of a term is that from which other senses are derived. The primary sense of a particular polysemous term is discovered by analysis of the relationships among the several senses of the term. The definition of a term in its primary sense may be specified in terms of the distinctive attributes of the class of things it designates (Scheffler and Lounsbury 1971:7–8). In the case of simple extensions, the class of denotata is either enlarged or contracted. However, a term may be applied to an essentially different designatum. If the application of the term to one designatum is derived from its application to another (i.e., the primary or simple extended sense), we may say that a transfer has occurred, and that the term is being used figuratively. Transferred senses may be differentiated from primary or simple extended senses through the discovery of derivation diachronically (etymologically) or synchronically (structurally). We may distinguish two general processes of semantic transfer: metaphoric, based on some perceived similarity between the primary (or simple widened) referent and the referent to which the term is transferred, and metonymic, based on some contextual association between the primary referent and the referent to which the term is transferred.

A metaphor is created when a linguistic label is transferred from its primary (literal) referent to a category that is perceived in some way as similar, but is distinctively different from the primary referent. The transfer connects the two referents so that a metaphor has two subjects—a principal one, which the term designates in its transferred sense, and a subsidiary one, which the term designates in its primary sense. The transfer accomplishes the designation of the principal subject and applies to it, as "associated implications," characteristics of the subsidiary subject carried along with the word from its primary meaning (Stern 1931:306; Black 1962:44).

A metaphoric transfer is accomplished by suspending at least one of the defining (distinctive) features of the primary designatum. "In its place some feature of connotative meaning which is associated with the primary or some simple widened sense of the term is made criterial for the metaphoric expression" (Scheffler and Lounsbury 1971:10). This formulation

accounts for the change in the significata of the primary and metaphoric designata of the term, but not for the richness of metaphor derived from the perceived similarities of the two designata. The distinctive features of the primary sense that are suspended from the significata (definition) of the metaphoric designatum do not become semantically inactive, but take on an "as if" quality (Lounsbury 1969:25). In other words, the meaning of the metaphor consists of the significata of the metaphoric designatum, which are made up of distinctive and nondistinctive features of the primary designatum, and, as connotations, those features distinctive to the primary designatum but nondistinctive to the metaphoric designatum. Further, connotations of the primary designatum may continue to be carried as connotations of the metaphoric designatum.

Metonymic transfer is a very different process from metaphoric transfer. Formally, a metaphor is accomplished by suspending at least one of the distinctive features of the primary (or widened) designatum, in place of which connotations of the primary sense become the distinctive features of the metaphoric designatum. In metonymy, however, there is no interaction of the distinctive features of the designata because there is no similarity. All the distinctive features of the primary designatum are suspended (becoming "as if" associations of the metonym), and the features of the designatum to which the term is transferred are substituted as distinctive. The key to understanding a metaphor is a knowledge of the *similarities* between the usual referent and the metaphoric referent. The key to metonymy is a knowledge of the *context* in which the usual referent and the metonymic referent are associated.

Whereas the primary sense of a term may be defined by the distinctive attributes of the class of things that it designates, figurative senses cannot be adequately characterized by the distinctive attributes of the designated classes because their senses are derived from the understood basis of the similarity or contiguity of two referents—literal and transferred. One may give the distinctive features of the transferred designatum, but one must also account for the relationship or interaction between the two referents. Thus, in metaphoric and metonymic transfers, it is not sufficient to specify the distinctive attributes of the designatum to which a term is transferred. As Black (1962) has argued, the substitution view of metaphor (or metonymy), in which a transferred term or expression is substituted for the usual or literal label of a given designatum, misses the point that in metaphor (and metonymy) the listener is forced to connect two ideas. That is, the listener must know distinctive and associated features of the primary designatum of the term and apply appropriate ones of these to the metaphoric designatum to organize his view of it: "The metaphor selects, emphasizes, suppresses, and organizes features of the principal subject by implying statements about it that normally apply to the subsidiary subject" (Black 1962:44–45).

The nature of transferred senses presents another problem for formal semantic analysis. The use of a term outside the domain of its primary sense as in metaphor and metonymy renders it no longer amenable to a precise accounting of its defining features. *Amma* in its primary sense "mother" belongs to a highly structured semantic domain (kinship), and

its defining attributes are discoverable by the contrasts which distinguish the members of the paradigm of kinship terms from each other. A metaphoric or metonymic transfer removes the term from its primary domain into an area in which terms and their senses are not usually paradigmatically related. Alterations in the semantic features of the designatum may be discussed, but the specification of the defining features is necessarily uncertain. This is probably an important property of most metaphor and metonymy, for it renders semantic definition uncertain and therefore open to interpretation.

With the concepts discussed in this section, functions of signs (referential and indexical), and the types of referential senses (primary, simple extended, and transferred—metaphoric and metonymic), we may proceed to the analysis of *amma* in its several senses.

THE ANALYSIS

We begin the analysis with the primary and widened senses of *amma*. Since the primacy of 'mother' becomes clear only in relation to the derived senses, its status as the primary sense will be established in the discussion of derived senses.

In designating genealogical relationships and in referring to individual kinsmen, *amma* always denotes ego's mother. In address, however, *amma* is used to grandmothers (FM, MM), senior parallel aunts (MeZ, FeBW), and husband's mother, as well as ego's own mother. These uses may be understood as simple extensions (widening) in which some oppositions (distinctive in the definition of the primary sense) are neutralized, thus generalizing the criteria for membership in the category (Scheffler and Lounsbury 1971:7,8,10). Mother, genealogically defined, is a kinsman (K), lineally related (L), in the first ascending generation (G+1), and female (♀), that is, K.L.G+1.♀. In using the term to address grandmother[6] (genealogically K.L.G+2.♀), the contrast between first and second ascending generations is neutralized so that *amma* signifies a kinsman (K), lineally related (L), in the first or second ascending generation (G+1 or +2), and female (♀), that is, K.L.(G+1 v G+2).♀.

In using the term to address senior parallel aunts, genealogically a kinsman (K) of the first ascending generation (G+1) older than the linking parent (+) first degree parallel collateral (C^1.//) and female (♀) (i.e., K.G+1.+.C^1.//.♀), the contrast between lineal and first degree parallel collateral, and the contrast of age within a generation (parent versus parent's elder sibling) are neutralized so *amma* signifies a female kinsman (K.♀), of the first ascending generation (G+1), whose age is equal to or greater than the linking parent, that is, whose age is not less than linking parent (~_), and who is a lineal or first degree parallel collateral relation (L or C^1.//), that is, K.(LvC^1.//).G+1.~_.♀.

Finally, in using the term to address husband's mother, the contrast between a female ego and her spouse is neutralized. This extension is

[6] Grandmothers and all other women of the second ascending generation may also be referred to and addressed as *ajji* or *ammaNNi*.

probably related to the change of membership from natal or father's family to husband's family that takes place at a woman's marriage. The neutralization in this case is different from those described so far in which contrasts in the componential definitions of designated kinsmen were neutralized. Here the contrast between ego and the linking kinsman, ego's husband, is neutralized. That is, a woman addresses her husband's mother with the same term he uses: *amma.*

Thus *amma* designates a category of kinsmen consisting of mother, and extended in address to senior parallel aunts, grandmothers, and husband's mother by the process of widening, in which contrasts between lineal and collateral, first ascending and second ascending generation, age within generation, and ego and ego's spouse are neutralized.

We may now consider the second class of senses of *amma:* the polite designation of women in reference or address. *Amma* 'woman, politely' is a metaphor. 'Woman, politely' and 'mother' are two senses of the term *amma.* 'Mother' is the primary sense. The metaphoric transfer is based on similarities between mothers and women entitled to some deference. Metaphoric transfer, it has been pointed out, is based on a similarity perceived between the referent of the term in its literal sense and the referent to which it is transferred. In South India, in what ways are women entitled to some deference similar to mothers?

In Avaruuru, virtually all adults are married. For a female, the transitions from child to adult, and from maiden to wife, are almost coincident. In the Kuruba caste, for example, a female puberty ceremony both marks the transition from child to adult and serves as an announcement that a young woman has become available for marriage. In the Adi Karnataka caste, it is considered good for a young woman to be married even before the puberty ceremony; here, the puberty ceremony signals that a girl is ready to assume her duties as a wife. A reason often given for the necessity of marriage at puberty is the power of sexual desire in young women, which must be held in check by parents (or trouble will surely result) until given its proper outlet in marriage. Kuruba families keep their unmarried adolescent girls at home.

The statuses of adult female and wife are inseparable (empirically if not analytically). As a wife, a young woman goes to live in the house of her husband. She is expected to treat him with deference. The respect she must give him is linguistically expressed: she refers to him as /nammejamaanru/ 'my (plural/polite) master,' and he is the only kinsman to whom she must refer with the plural/polite pronoun: /avaru/ 'they/he, polite.' A wife's respect for her husband is also shown in her behavior: she must walk behind him when they travel together, and eat after him when food is prepared (she may even eat the food left on his plate). These rituals of deference are observed by almost every woman. They express the distinctly subordinate status of a wife in relation to her husband. (The degree of actual subordination is highly variable, but the etiquette of deference is almost always observed.)

A young wife is subordinate not only to her husband, but to her mother-in-law (and elder sisters-in-law) as well. Her mother-in-law is in charge of running the household and allocates work to her daughters-in-law.

She oversees the tasks of food preparation and cleaning and may even supervise the sexual relations between her son and her daughter-in-law (e.g., seeing to it that post-partum taboos on sex are observed). With the mother-in-law, rituals of deference are fewer (perhaps because they work together): they converse freely; the wife addresses her mother-in-law as *amma* 'mother' instead of *atte* 'mother-in-law.' In terms of the organization of women's work, and the authority that a mother-in-law has over all her daughter-in-law's activities, it is quite clear that the daughter-in-law is the subordinate.

As a wife, then, a woman is subordinate, especially in her relationships with her husband and her mother-in-law. But it is a well-known pattern in the developmental cycle of Indian families for a wife's status to increase with time and the birth of children, especially sons. The desire for children is universal and strong. In the months after the consummation *(soobina)* of the marriage, women carefully inspect the bulk of the young wife's sari. Inquiries are often made, and an early announcement of pregnancy is greeted with pleasure and excitement. A mother is responsible for the care of her children (although under the aegis of her husband and her mother-in-law). They, in turn, owe her obedience and respect. Her daughters will leave the house when they become adults, but her sons will bring their wives to live and work under her supervision. As a mother, a woman assumes a position of respect and authority.

Thus, in Avaruuru, 'mother' and 'woman, politely' are similar categories. Adulthood, marriage, and motherhood are virtually coincident. It is from motherhood among these statuses that a woman derives respect and authority. Thus, *amma* used in polite address or reference designates a woman by the most respected status she can occupy. It is of no consequence if the denotatum is, in fact, a mother. The denotatum is female, and she is being denoted politely "as if" she were a mother.

The most straightforward indication that the primary sense of *amma* is 'mother' is the answer received on numerous occasions to the question /amma andreenu/ 'what does *amma* mean?.' The immediate answer was a synonym for mother /taayi/:/amma andree taayi/ '*amma* means mother.' No one answered this question with /heŋgasaru/ 'woman, plural/ polite' or an equivalent phrase. Further evidence also indicates that 'mother' is the primary sense of *amma*. This evidence comes from the single exception to the rule that *amma* may be used to address or refer to a woman politely: a man cannot address his wife as *amma* (e.g., /baamma/ 'come respected woman'), nor can he use her name with the *amma* form attached (if her name is *lakšmamma*, he will call her *lakšmi*). On one occasion this rule was cited with the explanation: /amma andree taayi samaana/ '*amma* means same as mother.' On the face of it, this is not a very helpful statement: Basically it says that mothers are mothers. However, it does imply that there is something about mothers which makes the term *amma* inappropriate for wives. In this instance, another man who was listening to the conversation provided further enlightenment when he laughed and said, /sokke, sokke aytu/ 'intercourse, intercourse occurred.' This comment clarified the basis of the implied opposition between mother and wife: with mother, sex is prohibited; with wife, it

is obligatory. The association of tabooed sex with 'mother' is also expressed in the obscene taunt: /nim amman hatra ootiiya/ 'your mother near do-you-go?'

We may infer from these examples that the association of a sex taboo with the category 'mother' results in the rule against the use of the term *amma* 'woman, politely' in reference to one's wife (with whom sex is obligatory). Thus the sense 'mother' is prior to the sense 'woman, politely.' Further, the example demonstrates that the sense 'mother' is not lost entirely when *amma* is used metaphorically to designate 'woman, politely.' It becomes what Black (1962) calls the subsidiary subject, by which, according to Stern (1931), the designatum, woman, is invested with certain associations. She is like a mother in that she is entitled to deference, and sexual access to her is prohibited by the incest taboo.

We may formally account for the metaphoric transfer of *amma* from 'mother' to 'woman, politely' in the following way. The componential definitions of the designata are K.G+1.L.♀ (mother) and ♀.A.p (woman politely, where A = adult, and p = politely). It is analytically true that all mothers are adult women. In the preceding section, I have shown that adult women are expected to be mothers, and that motherhood is a respected status. Therefore 'entitled to deference' is a connotation or associated implication of 'mother.' It is this connotation that becomes distinctive to the metaphoric designatum. But as it does so, it becomes both indexical and referential in meaning. The connotation of 'mother,' 'status entitled to respect,' is transformed into an indicator of the attitude of speaker to the addressee or referent, glossed 'politely.'

Formally, the features K.G+1.L. (the 'parent' part of mother) are suspended to an "as if" status, and the feature A (adult) remains as an analytic equivalent; the feature ♀(female) is carried over from 'mother'; and p (politely), having changed its sign function to indexical-referential, is substituted as a distinctive feature of the metaphor, so that the componential definition of the metaphoric designatum of *amma* 'woman, politely' is (♀.A.p). However, a metaphor gives us two senses for the price of one (so to speak), and this is not taken into account in the specification of the definition of 'woman, politely.' We must also include in our account the "as if" status of mother (K.G+1.L) with its implication of a sex taboo. Formally, then, the best we can do to represent the metaphoric sense of *amma* is: ♀.A.p, "as if" K.G+1.L., implying the incest taboo.

The basic sense of metaphoric *amma* is 'woman, politely.'[7] This sense is extended by widening to denote some other referents as well. That is, a metaphoric sense of a term, like the primary sense, may be extended by widening through the neutralization of contrasts in the defining features of the metaphoric designatum. *Amma* in its metaphoric sense is regularly applied to two additional categories of females. Several women reported that after scolding a young daughter, they would try to make up to her by saying affectionately /baamma/ 'come *amma*.' The designa-

[7] The basic metaphoric sense of *amma* is 'woman politely or deferentially'; the speaker's intention in using a deferential form may be to show respect, to express affection, or, if said sarcastically, to express contempt.

tion of a young girl as *amma* results in the neutralization of the contrast between adult and child, widening the sense of the term to 'female, politely.'

The other common extension of the metaphoric sense of *amma* is in address to a goddess (who may be graphically represented as ♀, altering the Venus symbol in keeping with depictions of Hindu goddesses who are equipped with multiple pairs of arms). In extending *amma* to goddesses, the componential definition is widened by the inclusion of goddesses with human females, thus neutralizing the implied contrast between them. Formally, *amma* 'goddess' is ♀.A.p.

The inclusion of goddesses as designata of *amma* 'respected woman' is best understood in the context of beliefs about women and goddesses. Goddesses are depicted in two ways: as consorts of male gods, and as independent deities. As consorts, goddesses are benign and subordinate; as independent deities (without husbands and children), they are fierce, powerful, and dangerous. Most South Indian goddesses, including Maariyamma, the village goddess of Avaruuru, fall in the latter category. Maariyamma is a powerful, easily angered goddess. In her anger, she causes smallpox (and, some believe, cholera and typhoid as well). When epidemics occur, she is worshipped in order to assuage her anger. But Maariyamma is also considered to be the protector of the village. She is worshipped annually after the harvest in order to evoke her benevolent care and avoid her destructive anger (see Beck 1971; Babb 1970). It seems likely that goddesses are addressed as *amma* and have names containing the *amma* form, like human women, both out of respect for their status as deities and in order to avert their wrath and evoke their beneficence. That is, they are called *amma* so that they will behave like well-socialized, respectable women, though powerful ones.

Ironic uses of metaphoric *amma* also occur. On one occasion, an old woman laughingly coaxed her crawling infant grandson, calling to him /baamma baamma/ 'come *amma* come *amma*.' By addressing her grandson as *amma*, she effectively neutralized all the oppositions that enter into the definition of the category, since her grandson was neither female, nor adult, nor entitled to deference. In the grandmother's usage, *amma* is transformed into affectionate irony by calling her grandson by a term that connotes the affectionate relationship between mother and child, but designates a category opposite to the one in which he belongs. On another occasion, a mother chided her son who was delinquent in his chores /baamma, baaree/ 'come *amma*, come (girl).' Her tone denied the affection and politeness (in addition to which, *baaree* is not a polite form), and her reference to him with forms signifying females shamed him.

Two senses of *amma* remain to be explicated: 'pox' and 'help.' The sense 'pox' is the result of a metonymic transfer, in which a word is used "to name a category in contextual association with the category usually named by the word" (Waldron 1967:186). In contrast to metaphor, which arises from some similarity perceived between the primary referent and the referent to which the word is transferred, metonymy arises from the contiguity of the referents. That is, in some context, the usual

referent of a word occurs in association with some other category. On the basis of that association, the word is used elsewhere to designate that other category (e.g., 'crown,' an object worn by a ruler, may be used to designate, metonymically, the office itself or the state).

In the case of 'pox,' the source of the metonym has already been alluded to. Pox is associated with goddesses: specifically, pox is a manifestation of the goddess (Reeve 1858:75; Ishwaran 1968:102–105). In its metaphoric sense, *amma* 'woman politely' is extended to goddesses in address. *Amma* 'goddess' is transferred from the designatum 'goddess' to the designatum 'pox.' Thus, the metonym *amma* 'pox' arises from the contextual association of pox with goddesses whose designation as *amma* is itself a widened metaphoric sense of the term. The complexity of this case in which a metonym is derived from an extended metaphoric sense of a term provides an example of the rich and varied ways in which the senses of a term may be related to each other.

The data are insufficient for the specification of the distinctive attributes of pox, which must therefore remain unanalyzed. The distinctive features of the metaphoric sense from which the metonym was derived ('woman-goddess politely' ♀.A.p), and the distinctive features of the primary sense of *amma* ('mother' ♀.K.G+1.L), are the filters through which 'pox' is perceived. Thus, the metonymic designation of pox as *amma* carries associations of the ambivalent, destructive-benevolent nature of its cause, the goddess (Babb 1970; Beck 1971; Ishwaran 1968).

One sense of *amma* remains to be related to the others: the cry of distress or surprise. Thus far we have discussed literal, simple extended, and transferred senses of *amma*. We have shown how literal and figurative senses can be generalized by widening to include additional denotata, and how transfers are effected from the literal designatum to a figurative designatum on the basis of similarity (metaphor) or contextual association (metonymy) of the designata. That is, so far we have been dealing in sign functions which are referential or primarily referential with an indexical element.[8] *Amma* 'help,' a cry of distress or surprise, is, by contrast, primarily indexical. We are confronted not with a transfer from one designatum to another, a change in reference, but rather a change in sign function, from referential to indexical. Since the shout *amma* is a manifestation of its utterer's distress, it is a sign that is related to its object by existential association rather than by reference to it. Thus *amma! amma!* does not refer to a state of distress or surprise; it is a manifestation of that state.

Because there is no referent, the cry of distress cannot be analyzed in terms of widening or transfer from the primary referent to some other referent. Instead, we must account for the transfer from a primarily referential to a primarily indexical sign function. The transfer is much more difficult to analyze than those semantic derivations discussed so far be-

[8] Earlier, we saw that metaphoric *amma* included the referential-indexical feature 'politely.' Further, the meaning of vocative *amma* is not solely referential, since it both refers to a denotatum (referential) and calls that denotatum (indexical).

cause there is no referent whose attributes can be described, and perhaps because of the essential nature of expressives. I will argue that like other expressives, *amma,* the cry of distress, derives its efficacy from the power of the word itself, that is, from its several primarily referential senses and their connotations. Expressives are potent because their referential senses designate that which is taboo (e.g., damn!, Jesus Christ!). It seems that in many languages, kinship terms also fall into this category—for example, in addition to Kannada *amma,* note Italian *mamma mia!* and English 'oh brother!'

The efficacy of *amma* as an expletive derives from the association of *šakti,* the female cosmic force, with the categories designated by *amma:* mothers, goddesses, and women. Among female beings, goddesses are the most powerful. As we have seen, goddesses may be divided into two classes: married goddesses, mild-mannered and benevolent, who are subordinate to their husbands; and unmarried goddesses, who are powerful and potentially destructive. Maariyamma, the protector of Avaruuru and the goddess of smallpox, is of the latter type. She is a fierce and feared goddess. Her terrible power, when uncontrolled, can cause smallpox, but her power can be harnessed for the benefit of human society through worship and blood sacrifice. This dual nature of goddesses (married and benevolent, single and dangerous) is an aspect of the character of human females as well. The power of human females, like that of goddesses, manifests itself when the females are without proper social statuses. Young unmarried women, widows, and barren women are feared. Young unmarried women, we have seen, possess a powerful sexuality which, uncontrolled, can wreak havoc with the social system by producing children without kinsmen. Fortunately, this potential danger can be contained by parents until the young woman is placed firmly in the control of her husband's family.

A barren woman's power manifests itself in a different way. A woman who fears she is barren will make vows to deities, perform rituals, go on pilgrimages, and visit medical practitioners in the hope of inducing pregnancy. In Avaruuru, barrenness is the only widely accepted reason for taking a second wife. Such a marriage is a public announcement that the first wife is inadequate, a failure at her most important task as a wife: to produce children, to become a mother. Barren women are envious. Their envious glances transmit the evil eye *(dišTi),* and they are considered angry and powerful enough to cause significant harm to others.

A widow is the most dangerous of all mortal women. A woman who is widowed loses much. If she is young and has children, she suffers from the loss of a provider and must depend on her husband's brothers, or on her own brothers, for whom she and her children are, at best, secondary obligations. She may be forced to fall back on her own resources to provide for her family, perhaps by working in the fields. If she is middle-aged, she is in a better position for she will have sons to provide for her. But, unless strong, she may find that when the status of head of house passes to her son, her position as mistress of the household will

be lost to her son's wife. Further, as a widow, she is barred from participation in many of the important events of social life. She is no longer allowed to participate in weddings: for example, the selection of a bride, and the ritual bath given the bride and groom before the ceremony. In addition, she loses many of her prized possessions. Ornaments that both enhance feminine beauty and indicate wealth and status are taken away, and she is forbidden to put them on again. Some of these were acquired in girlhood, some at marriage and after. The signs of marriage, her *taali* (gold disc) and her toe-rings, are taken from her at her husband's funeral. The *taali*-string is cut, and the toe-rings removed. Further, the red mark worn on the forehead since girlhood is henceforth forbidden, and the glass bangles that have adorned her arms since childhood are broken and may never be replaced. A woman loses much of her position in society when she loses her husband: her place in the household, the right to participate in social events, and the privilege of adorning herself. As a woman outside of society, she is feared. Her power is no longer kept in check. In places not far from Avaruuru she is suspected of witchcraft (Harper 1969).

Thus we see that women without husbands and women without children are feared. Their female power *(šakti)* is not channeled into the proper social roles—wife and mother—and unleashed, it is a powerful and dangerous force. Maariyamma, like barren woman and widows, has no husband and no children. She is like a barren woman or a widow but much more powerful, and therefore potentially much more dangerous and destructive.

Šakti, or cosmic power, is most obvious as a dangerous force when, as in the goddess of smallpox or the widow, it is not socially controlled. But *šakti* can be harnessed by placing females in their proper social roles as wives (of gods or men); when it is properly channeled, *šakti* is benevolent, as in the case of the goddess who protects, the wife who looks after her husband, the mother who cares for her children. Even the destructive force of *šakti* represented in the terrible goddess Maariyamma can be harnessed to destroy evil rather than good. *Šakti*, then, is dangerous when it is asocial (in unmarried goddesses, barren women, unmarried girls, and widows), but it can be socialized and made benevolent by worship, by marriage, and by motherhood. The expressive power of *amma* 'a cry of distress or surprise' is derived from the association of potentially beneficial power with all the female beings designated by *amma* (mothers, respected women, and goddesses). It is this which makes *amma* good to shout.

CONCLUSION

We identified in the meanings of *amma* a linguistic sign functioning in two ways: referentially and indexically. Using the concept of polysemy in referential signs developed in ethnographic semantics, we identified the primary, widened, and transferred referential senses of *amma*. From the discussions of transferred senses in linguistics, literary criticism, and philosophy, two major principles of derivation were identified: similarity

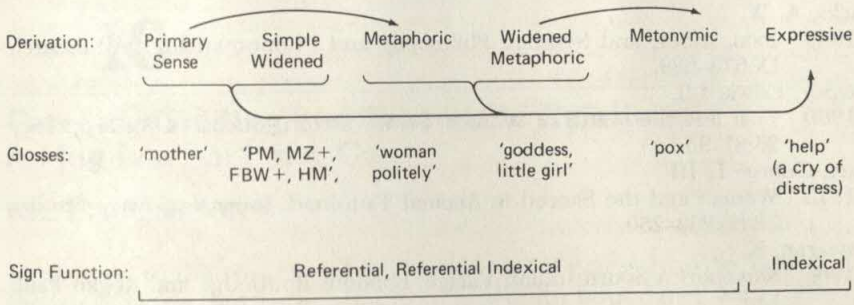

FIGURE 1. The relationships among the several senses of *amma*.

as the basis of metaphor, and contiguity as the basis of metonymy[9] (Lounsbury 1959; Black 1962; Waldron 1967; Jakobson and Halle 1956). Using these concepts, we were able, systematically, to account for the relationships among the several senses of *amma*, summarized in Figure 1.

Thus, by adding the notion of nonreferential sign functions, and by using the concepts of metaphor and metonymy already alluded to in the literature, we were able to build on the analytical procedures of ethnographic semantics to account systematically for interdomain and extradomain senses of a term. In order to analyze the metaphoric and metonymic transfers, and the change in sign function from referential to indexical, we had to discover the cultural basis of the conventional similarities (between mothers, respected women, and goddesses) and the conventional associations (between goddesses and smallpox, mothers and a sex taboo, females and *šakti*). Thus, in analyzing the meanings of *amma*, we have exposed a set of concepts about women and goddesses in the culture of Kannada speakers. The results of this analysis, therefore, should be relevant to both ethnographic semantics and South Asian ethnography.

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[9] It has been noted that similarity and contiguity seem not only to be the principal modes of derivation in referential transfers, but also the principles on which Peirce's (1940) icon and index are based; an index is existentially related to that which it denotes, whereas an icon is similar to that which it denotes. Further, in linguistics, the concepts of paradigmatic and syntagmatic relations are likewise based on the principles of similarity and contiguity. Farther afield, Fraser identified two classes of magical acts: sympathetic acts based on the principle of similarity, and contagious acts based on the principle of contiguity. It appears that similarity and contiguity are two powerful principles of organization for human thought (Jakobson and Halle 1956; Lounsbury 1959).

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10

Category Grading and Taxonomic Relations: A Mug Is a Sort of a Cup

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In contrast to the traditional academic view of categorization, casual folk theory suggests that categories have indefinite boundaries and grade off into one another. Some categories, such as "liberals" and "tall women," seem inherently graded; but any category can be graded when necessary. Until recently, such folk wisdom has been ignored by cognitive anthropologists, and categories have been treated as sharply bounded sets specified by independent criterial features. The assumption of these properties facilitates the elicitation of folk classification systems and simplifies our formal description of those systems. However, these assumptions are now being questioned as a result of recent work in several disciplines.[1]

This paper uses a formalism known as "fuzzy set theory" to relate gradations in category membership to the taxonomic relations of inclusion and contrast. Interviews eliciting graded identifications of objects are compared with standard taxonomic interviews. The two types of interviews explore only a limited domain—containers for liquids—and examine in detail six categories in this domain.

In most cases, the two types of interviews produce quite compatible results; category gradations simply give a more detailed view of the taxonomic relations we already understand. However, in a few particular cases, the informant's taxonomic statement "X is a kind of Y" glosses a relation that violates fundamental requirements of standard taxonomic theory. Although fuzzy set theory gives a clearer view of this problematic inclusion, it is not correctly defined by fuzzy set theory either. I describe the modification necessary to explain my data and argue that so modified, fuzzy set theory offers a more nearly complete and accurate means of describing human cognition than has previously been available.

OTHER LITERATURE ON CATEGORY GRADING

Variants of category grading have been applied to a number of problems in diverse fields. In this section I review several lines of research that apply the same basic ideas under various rubrics.

Sapir, in a 1944 article, discusses the use of quantifications in langauge. He delimits four degrees of grading, the least explicit being "implicitly gradable but ungraded":

> Every quantifiable, whether existent (say *house*) or occurrent (say *run*) or quality of existent (say *red*) or quality of occurrent (say *gracefully*), is intrinsically

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† Source: *American Ethnologist* 5:44–65, 1978 (revised).

> gradable. No two houses are exactly identical in size nor are they identical in any other feature that can be predicated of them. Any two houses selected at random offer the contrast of "more" and "less" on hundreds of features which are constitutive of the concept "house." Thus, house A is higher but house B is roomier, while existent C is so much smaller than either A or B that it is "less of a house" than they and may be put in the class "toy" or at best "shack.". . . And "gracefully" is quite unthinkable except as implying a whole gamut of activities which may be arranged in a graded series on the score of gracefulness. Every quantifiable, then, not yet explicitly quantified, is gradable. Such terms may be called *implicitly gradable but ungraded* (1968 [1944]:123).

But he discusses further only the more explicit forms of grading, such as "more of the house" or "a large house." In Sapir's terms, I am here concerned with discovering the implicit grading of existents.

Zadeh has developed a formal system, *fuzzy set theory,* that provides a *membership value* to characterize the relationship between an element and a set (Zadeh 1965, 1971, 1973). Thus, things are not simply members of a set or not members of a set, but rather members to a greater or lesser degree. I use this convention to assign numerical values to informant judgments of "more membership" and "less membership," thus allowing algebraic treatment of category membership. Lakoff extends the idea of set membership value to *degrees of truth* of sentences. He analyzes hedges (phrases such as "sort of," "more or less," and "strictly speaking") in terms of fuzzy logic, proposing several different types of criteria for category membership (Lakoff 1973, also see Zadeh 1972).

Rosch discusses a similar notion, the *internal structure of categories:* "Categories are composed of a 'core meaning' which consists of the 'clearest cases' (best examples) of the category, 'surrounded' by other category members of decreasing similarity to the core meaning" (Rosch 1973a:112). She has applied this concept to variation and formation of color categories (Heider 1971, 1972; Heider and Oliver 1972), geometric shapes, and "semantic categories" (Rosch 1973a, b). In carefully controlled experiments, Rosch has demonstrated that subjects agree on rankings of exemplariness of things in categories; these rankings are consistent both with measurements of speed of judging category membership and with child errors in judging category membership. More recently, Rosch has investigated differences between categories at different levels in taxonomic systems (Rosch et al. 1976) and the extent to which prototypical members share attributes (in "family resemblances") with other members of a category (Rosch and Mervis 1975).

Using drawings of "cups and cuplike containers," Labov documents shifts in the boundaries of the meanings of words such as *cup, bowl, dish, mug,* and *vase* in different contexts. His "measurement of vagueness" depends upon agreement among speakers in use of a term (1973:353); thus, Labov indirectly measures category membership by percentage of speakers. Anderson extends this approach and demonstrates developmental changes in children's category boundaries and features of these categories (1975).

Kay and McDaniel (1978) replace the earlier Berlin and Kay (1969)

notions "focus" and "boundary" of color categories with a fuzzy set formulation, which maps features such as wavelength into degrees of membership in color categories. They apply fuzzy set union, intersection and inclusion to color categories and develop formulations for mutual exclusion and partition. Kay and McDaniel argue that fuzzy set theory provides a more appropriate means of discussing the universality and encoding sequence of basic color categories than did the earlier description of focus and boundary. In an earlier work, Kay (1975) demonstrates that the accepted definition of inclusion in fuzzy set theory does not offer a solution to problems of intransitivity in folk taxonomies.

These disparate approaches, taken together, provide a basis for the research I report here. Rosch lays the groundwork in psychological testing by demonstrating that a general concept "amount of category membership" is valid and replicable across several different measurement techniques. Zadeh provides a formal means of expressing this concept and makes testable assertions about relative membership values in taxonomically related categories. Sapir and Lakoff illustrate easily observable linguistic manifestations of category grading; I use Lakoff's "hedges" for elicitation of membership values. Labov offers insight into a domain similar to the one I investigate here and provides independent verification of some of my data. Finally, Kay and McDaniel relate fuzzy set theory to ethnographic semantics and deal with theoretical problems related to those discussed here.

DATA COLLECTION

Classificatory data were elicited in two types of interviews. The first was a standard taxonomic interview referring only to category labels. This interview utilized the question frames "What are the different kinds of _____?" and "What is _____ a kind of?" Using such question frames, ethnographers of folk classification would infer from an informant statement such as, for example, "apples and pears are different kinds of fruit" that the categories *apple* and *pear* are in direct contrast. Similarly, such a statement permits the inference both that the category *apple* is included in the category *fruit* and that *pear* is included in *fruit*.

The second set of interviews elicited grading of category membership. In these interviews, informants were presented with both objects and graded statements—they were asked to choose the statements which best related the objects to the categories. This "hedged interview" followed the standard taxonomic interview several hours to several days later, and the same informants were used.

The informants were students and former students at the University of Texas in Austin; no particular attempt was made to interview individuals from differing subcultural groups.

The Standard Taxonomic Interview

The standard taxonomic interviews elicited terms that denoted categories related by inclusion and direct contrast to the term *cup* and followed standard ethnoscience prescriptions for this task (compare Tyler 1969:1–

25; Frake 1969:28–41). At the beginning of the interview, I provided an example of the desired relations among terms by suggesting that a possible answer to "What are the different types of vehicles?" might be the list: *cars, buses, trucks, planes,* and so forth. During the interview, I was careful not to suggest any other terms (aside from the starting point, *cup*). Informants were not asked to continue listing terms beyond what they volunteered.

Eleven informants were given these standard taxonomic interviews, starting with "cup" and working toward both more specific and more general terms. The informants volunteered five taxonomic levels. Two were more specific than *cup* (included in, preceded by, or subsets of *cup*), and two more general than *cup*. The data from these interviews are presented in Figure 1, with the usual convention that lines connecting category labels signify categories related by inclusion. The numbers to the left of each line indicate the number of informants (out of eleven) who reported that inclusion relation.

In constructing the diagram of Figure 1, only terms mentioned by three or more informants are included. In some cases, it seems that the same term is used to refer to different categories. For example, *utensil* is mentioned five times as a category that includes *dishes* and one time as a type of *dish*. *Mug* is mentioned nine times as a type of *cup* and twice as a type of *coffee cup*. Some objective evidence indicates that *brassiere cup* should not be included in the same domain; the three informants each mentioned it after listing other types of cups, and immediately before or immediately after a pause; it was always accompanied by a laugh, and one informant spontaneously said that it "is different."

There was less agreement on the terms naming the category that immediately included *cup* than on the types of *cups*. Five informants mentioned

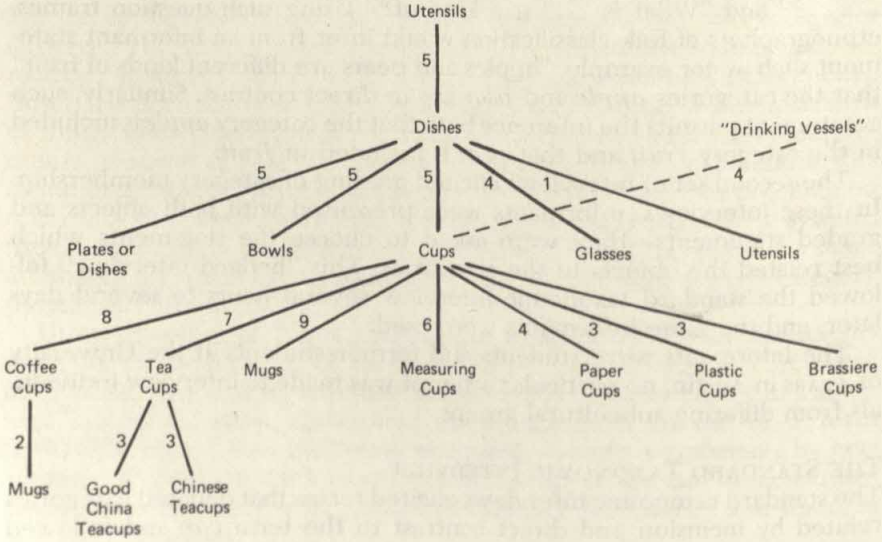

FIGURE 1. Category relations and labels elicited in the standard taxonomic interview.

dishes (often providing the example phrase "hafta do the dishes"), while one informant mentioned each of the following: drinking vessel, utensil for hot liquid, tableware, glasses, and drinking utensils. Although no label is standard, approximately half of my informants named first a category of "things to drink out of" (in place of *dishes*) as the category including *cup*. Figure 1 indicates this by the gloss *drinking vessel* and the dotted line. If asked, informants who chose either *dishes* or *drinking vessel* as the superset of *cup* agreed that the other could be used. The salience of *cup*, its status as a primary lexeme, and other factors place it at the generic rank, or basic level, in the taxonomy (Berlin, Breedlove and Ravin 1973, Rosch et al. 1976).

Within contrast sets, some terms were listed by more informants and were mentioned earlier by each informant listing them. Therefore, a convention is introduced in Figure 1 to represent this regularity: categories in direct contrast are ordered left to right on the basis of informant ordering and frequency.[2] Tree diagrams presented in accounts of folk systems of classification often seem to have this left-to-right ordering, but it is not explicitly mentioned in the literature. For example, Berlin discusses a regularity in plant taxonomies involving polysemy between a specific and generic; these taxa (categories) are described as "the type-specific, the archetype, or the ideal type" of their generics (Berlin 1972:59), and they are always put leftmost in his key diagrams (1972:60–68).

The Identification of Objects in the Hedged Interview

The frequencies and the consistent ordering in lists suggest that informants were grading category membership even in the standard taxonomic interview. To obtain more detailed data on grading, informants were asked to use hedged or absolute statements to make explicit judgments of membership value. These interviews used hedged statements similar to those of Lakoff (1973). However, rather than using a hedged statement to relate two category labels, as in "a chicken is sort of a bird" or "a robin is a bird par excellence" (Lakoff 1973), I presented physical objects and asked informants to choose the most appropriate statement to relate each object to a category label. This task, the process of relating a new object to a known label, is properly referred to as *identification* rather than classification (see Sokal 1974, Hunn 1975).

The objects were fifty-one small ceramic containers, which were randomly numbered and presented to informants as a set. In an earlier sorting and naming task (not reported here), other informants referred to these objects as *mugs, cups, pitchers, jars,* and *bowls.* Because this paper presents the first phase of a study comparing American English and Mexican Spanish, the objects were purchased in Mexico and included some shapes that were unfamiliar to some of the informants in this English-speaking group.

The hedged identifications were carried out by nine of the eleven informants who completed the standard taxonomic interview.[3] Since all informants were familiar with testing techniques in the contemporary United States, they were presented with the questions in written form

and wrote their answers on multiple-choice answer sheets. I was present during the interviews and encouraged questions and comments.

Each of six question sheets asked about one of the following categories: *pitcher, cup, mug, coffee cup, jar,* and *drinking vessel.* On each category sheet, fifty-one blanks referred to each of the objects. Since the sheets were presented one at a time, to "identify" an object, *the informant did not have to choose among categories.* Thus, informants were not asked whether an item was a *cup* or a *drinking vessel* or a *pitcher*—rather, they were asked how much it was a *cup,* then how much it was a *drinking vessel,* and then how much it was a *pitcher.*

On the basis of previous work with footwear categories, I selected six grades of hedged phrases that denote increasing amounts of membership in categories:

(0) absolutely not a _____;
in no way a _____.
(1) not really a _____, but a _____ in some ways;
in one way a _____.
(2) sort of a _____;
kind of a _____;
in the _____ family.
(3) primarily a _____, but not completely.
(4) a _____.
(5) the most typical _____, or one of the most typical _____s; the best example of a _____, or one of the best examples of a _____.

Almost all of the informants felt that the hedged phrases "made sense." In some cases, they would use one of the alternative phrases (for example, to one informant, "in the cup family" did not make sense, but he chose the membership grade "2" to identify the object as "sort of a cup"). Two informants felt that the ordering of hedged phrases could have been different; each suggested that two adjacent grading phrases on the scale could have been reversed, but these informants said they could "also see it" as ordered on the questionnaire. No informants suggested that nonadjacent phrases could be interchanged. In sum, judging by informant comments during the interview, the category membership statements were reasonable and appropriate to my informants.

Frequency of Hedged Answers

As another check, simple counts of answer frequencies were employed to verify whether the hedged statements used in the interviews were reasonable and appropriate. The distribution of answers to the category questions (see Table 1) reveals that hedged phrases were indeed chosen frequently. Thus, we conclude both that the hedged phrases made sense, and that categories in this domain are not sharply bounded.

Table 1 shows that informants frequently chose hedged statements to identify the objects. In addition, phrases denoting higher grade of membership were selected less frequently. A higher proportion of negative statements is expected simply because the statements referred to several contrasting categories.

TABLE 1. Frequency and Proportion of Answers Using the Various Graded Statements

	Number of Answers	Percentage
(0) absolutely not a _____	1097	40
(1) in some ways a _____	558	20
(2) sort of a _____	446	16
(3) primarily a _____	229	8
(4) a _____	338	12
(5) best example of a _____	83	3

Although the overall distribution is fairly even, some informants used particular phrases infrequently. One informant mentioned at the beginning of the interview that he was not ever going to mark "the most typical _____, the best example of _____" because he disagreed with the concept of Platonic ideal types. He was the only informant who never used one particular graded statement, but two other informants used the "best example" phrase less than four times, and two informants used the "primarily a" phrase less than four times. The low proportion of "best example" grades was in part due to the choice of objects used in the interview—many informants stated that my set of objects did not include "best examples" of some of the categories, although they could visualize such best examples. This was sometimes put in the form of a description of what the best example of the category would have looked like, had it been present.

Besides verifying whether the questions were appropriate, the distribution of answer frequencies can also provide a crude means of checking taxonomic relations among categories. This distribution is presented in Table 2. For simplicity, the six possible grades are collapsed into three (not a member, partially a member, and a full member).

The number of "a member" responses clearly depends upon what objects were used in the interview. For example, we would conclude from Table 2 that informants were shown more drinking vessels than jars. But in addition, taxonomic relations among categories impose certain restrictions on the relative frequencies, independently of the objects being used. By definition, any member of a category is also a member of

TABLE 2. Percentage Distribution of Membership Grades Across Category Terms

	Not a Member	Partially a Member	A Member
pitcher	58.5	26.6	14.8
cup	56.7	30.4	12.9
mug	54.5	33.2	12.2
coffee cup	69.5	17.4	13.1
jar	76.2	15.1	8.8
drinking vessel	45.3	24.7	30.1

its superset. Thus a category can not have more "a member" responses than its superset (for example, there cannot be more cats in a room than there are animals in it). Further, since categories in direct contrast are mutually exclusive, the sum of "a full member" responses for contrasting categories should not be greater than the number of positive responses for the category that includes both of them.

Comparing Figure 1 with the "a member" column of Table 2, we see first that *cup, mug,* and *coffee cup* all have fewer "a member" responses than their superset *drinking vessel,* as expected. Further, the combined positive responses to the directly contrasting *mug* and *coffee cup* should not be greater than either the positive responses to their superset *drinking vessel,* or their superset *cup.* This prediction is realized for *drinking vessel:*

Expected: mug + coffee cup $\leqslant$ drinking vessel
Observed: $12.2 + 13.1 \leqslant 30.1$

It is however, far from correct for the immediate superset *cup:*

Expected: mug + coffee cup $\leqslant$ cup
Observed: $12.2 + 13.1 > 12.9$

Recall that in the standard taxonomic interview my informants were consistent in listing *mugs* and *coffee cups* as distinct kinds of *cups;* the different findings of these two interviews are quite inconsistent with our notions of inclusion and contrast.

If the above description does not seem problematic to the reader, I will point out that this inconsistency is logically equivalent to having my informants state that the number of animals in a room is less than the number of dogs plus the number of cats. Data of this sort present a serious problem for standard taxonomic theory. The relationships between the categories *cup, mug,* and *coffee cup* will be examined in more detail later, and an attempt is made to understand why these anomalous results have been obtained. But before returning to this problem, we shall examine taxonomic inclusion from the viewpoint of fuzzy set theory. Although these anomalous category relations are also problematic for fuzzy set theory, this theory shows more clearly where the problems lie.

COMPARISON OF TAXONOMICALLY RELATED FUZZY CATEGORIES

This section introduces graphs to examine relations between categories. Papers on fuzzy set theory normally graph feature values against membership values; here, however, membership values in one category will be graphed against membership values in another category. Each observed relation between categories will be compared with informants' explicit statements about taxonomic relations.

On the basis of the standard taxonomic interviews described earlier, and supplementary questioning about *jar, pitcher,* and *drinking vessel,* the taxonomic relations between categories are fairly clear:

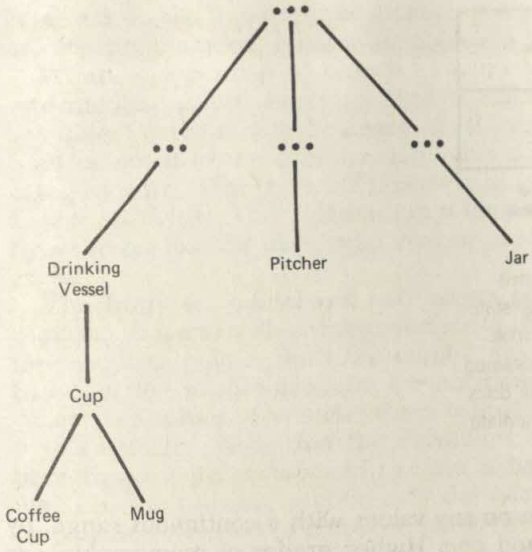

FIGURE 2. Taxonomic relations among the categories used in the hedged interview.

Despite lack of agreement on what terms should precede *pitcher*, and *jar*, these two definitely contrast with *drinking vessel* and its subsets. The term *drinking vessel* was selected rather than *dishes* to name a superset of *cup* because the polysemy of *dishes* probably would have confused informants (see Figure 1). Before examining the data, I will outline what fuzzy set theory leads us to expect for the relations of inclusion and contrast. Hypothetical data on the categories *vehicle, car* and *truck* will be used as an illustration.

WHAT FUZZY SET THEORY PREDICTS

In standard set theory, given the sets X and Y and some object x, a statement of taxonomic inclusion, such as "X is a subset of Y," "X is included in Y," or "Y is the immediate superset of X," is equivalent to the statement "If x is a member of X, than x is necessarily a member of Y." Taking as an example the categories *car* and *vehicle* and a set of objects $x_1, x_2, x_3 \ldots x_n$, I draw out the consequences of the claim *"car is a subset of vehicle"* in the table in Figure 3. That is, an object can fall into cell A, B, or C, but no objects can fall into the upper left cell (because nothing can be a car and not also a vehicle); the upper left cell is thus denoted by the symbol for the null set, ϕ.

Set inclusion is defined similarly in fuzzy set theory. Given a universe of objects and the fuzzy subsets *vehicle* and *car* within this universe, any object x in the universe can be characterized by its membership value in these fuzzy subsets. The Greek letter μ is used to denote membership value. The membership value of x in the fuzzy subset *vehicle* is here denoted $\mu_{vehicle}(x)$; in the fuzzy subset *car*, $\mu_{car}(x)$. These membership

CAR	Is not a vehicle	Is a vehicle
Is a car	Ø	A
Is not a car	B	C

VEHICLE

FIGURE 3. Possible memberships predicted by standard set theory if the statement *car* is a subset of *vehicle* is true. In such tables set membership is assumed to be either true or false; this table does not allow representation of intermediate grades of membership.

values are permitted to take on any values with a continuous range, by convention between zero and one. Higher grades of membership are assigned values closer to one. Thus, relationships between membership values can be represented on a graph. Figure 4 presents a hypothetical graph of an inclusion relationship. Such a graph might be obtained by asking an informant to what degree each of a set of objects is a vehicle and to what degree it is a car; each point represents one object. If data were obtained such as those in Figure 4, we would conclude that *car* and *vehicle* are categories that do not possess intermediate values.

The graph of Figure 4 is essentially the equivalent of the table in

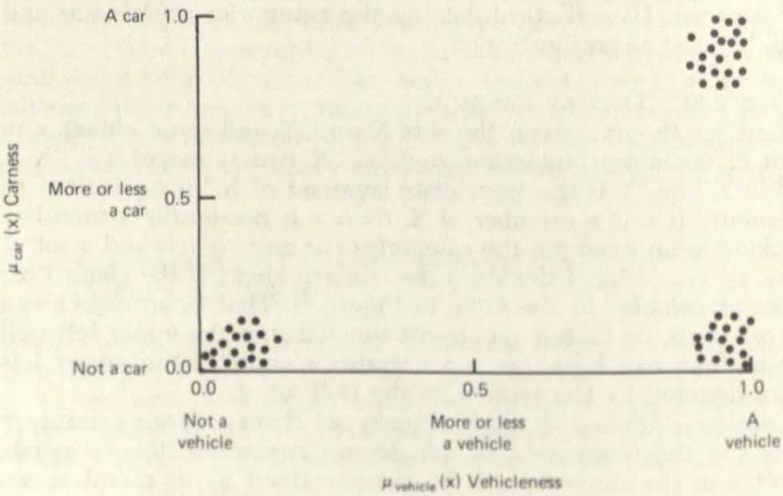

FIGURE 4. Hypothetical data demonstrating the inclusion of *car* in *vehicle* if these are sharply bounded categories. On a fuzzy set membership graph, discrete categories appear as clusters of points separated by distance.

Figure 3. In the hypothetical situation of Figure 4, where the categories are sharply bounded, a fuzzy set approach is unnecessary.

When membership of objects in fuzzy subsets does in fact take on intermediate values, fuzzy set theory defines the membership value of any object in the superset category (in this example *vehicle*) to be greater than or equal to the membership value of any object in the included category *(car)*. That is, for all objects x: $\mu_{vehicle}(x) \geqslant \mu_{car}(x)$ (Zadeh 1973:31, Lakoff 1973:461). This relationship is illustrated in Figure 5, in which I hypothesize that the categories are continuously graded, rather than discrete.

The fuzzy set model not only allows points to fall in intermediate positions, but also makes statements about the expected locations of these intermediate points. Both the empty upper diagonal in Figure 5 and the equation $\mu_{car}(x) \leqslant \mu_{vehicle}(x)$ are notational variants of the statement "if car is a subset of vehicle, there is no object that is more a car than it is a vehicle." Note that the definitions of fuzzy set theory are used here to compare *membership values* as being "less than or equal to," while in the previous section only the *relative number* of "a member" responses were used to compare categories.

The case of contrast between fuzzy sets is analogous and can be derived.[4] When two categories are in direct contrast, the membership values of any object in these two categories cannot have a sum greater than the maximum allowed membership value (by convention, 1.0). Thus a contrast relation, which might be expected between *car* and *truck*, can be described by the equation: $\mu_{car}(x) + \mu_{truck}(x) \leqslant 1.0$. This is illustrated in Figure 6. In other words, in this relationship of contrast, an object

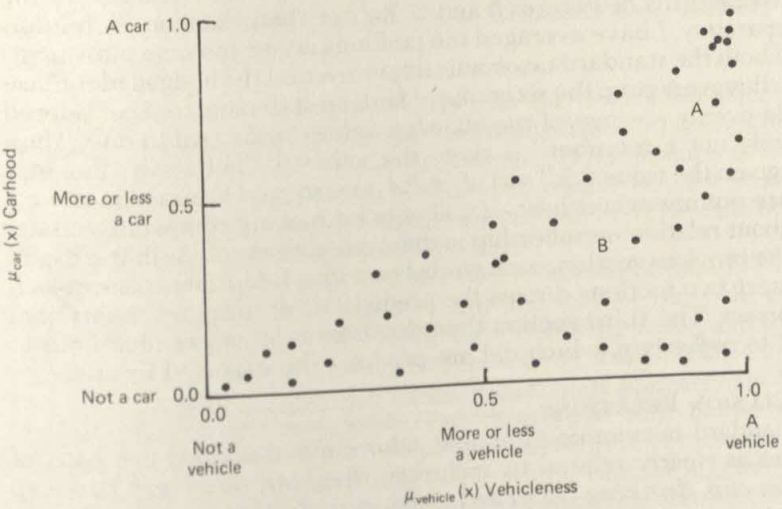

FIGURE 5. Hypothetical data on *car* and *vehicle* if these are continuously graded categories. For example, a VW bus might fall at point A and a soap box derby winner at point B. Fuzzy set theory predicts that if *car* is a subset of *vehicle*, all points will fall on or below the diagonal.

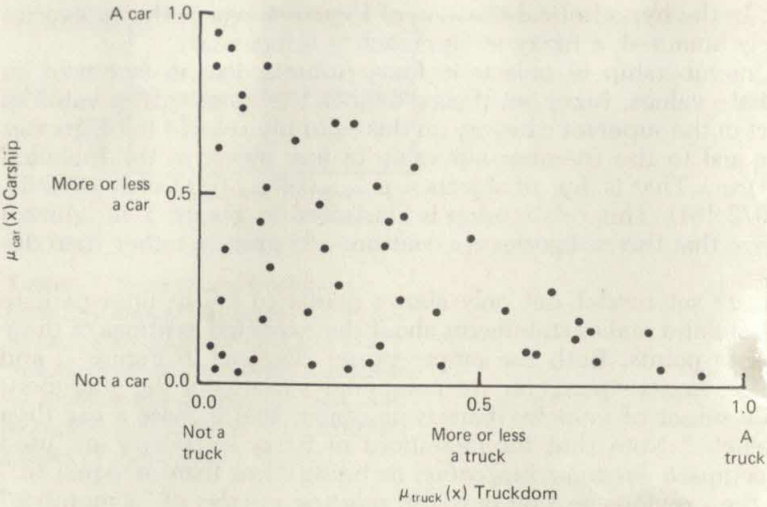

FIGURE 6. Hypothetical data illustrating contrast between the categories *car* and *truck*.

can be partly a car and partly a truck or it can be neither a car nor a truck, but it cannot be fully a car and fully a truck, nor can it be fully a car and partly a truck, and so forth.

PRESENTATION OF THE DATA

The hedged identifications can now be discussed with reference to the predicted patterns of Figures 5 and 6. Rather than examine each informant separately, I have averaged the rankings across the nine informants who did both the standard taxonomic interview and the hedged identifications. In this averaging, the six grades of hedged statements are considered to divide evenly a range of membership values from zero to one. Thus, "absolutely not a member" is given the value 0, "not really, but in a way" is given the value 0.2, "sort of" is 0.4, and so on. The exact numerical values are not important here—I will only be making comparative statements about relative membership in different categories. As in the examples of the previous section, each symbol on a graph represents one object.

The next two sections discuss the predictions of fuzzy set theory that were correct. The third section then discusses relations of *mug* both to *cup* and to *coffee cup,* which did not produce the expected results.

THE INCLUSION RELATIONS

In the standard taxonomic interview, informants described five pairs of categories as clearly related by inclusion: *drinking vessel* and *cup, cup* and *coffee cup, drinking vessel* and *coffee cup, drinking vessel* and *mug,* and *cup* and *mug.* Of these, all but *cup* and *mug* produced essentially the results expected from fuzzy set theory. Discussion of three of these expected relations will sufficiently illustrate inclusion of fuzzy sets.

Figure 7 graphs *drinking vessel* with its subset, *cup.* Identifications

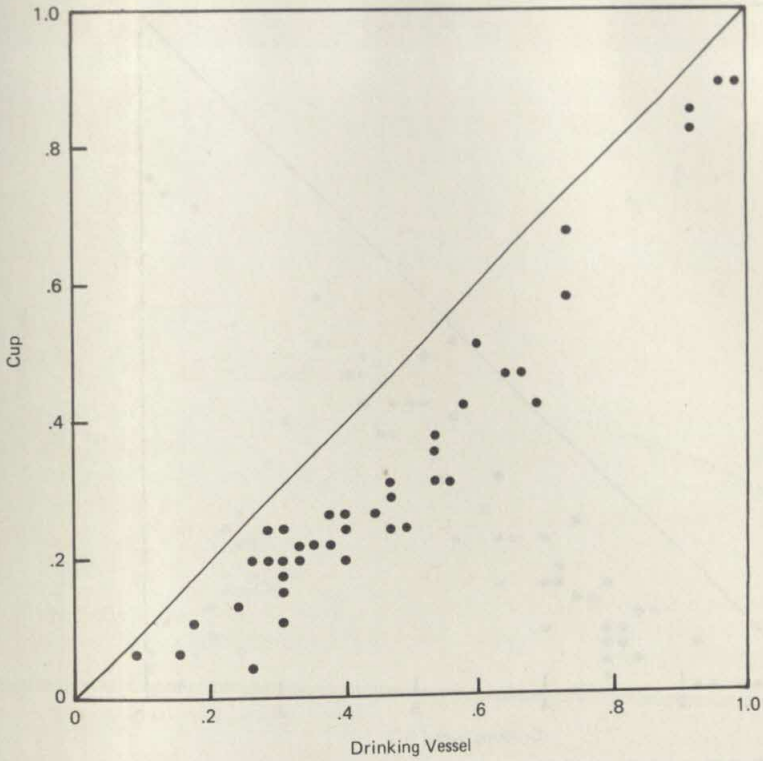

FIGURE 7. Elicited data demonstrating that under the fuzzy set theory definition, *cup* is a subset of *drinking vessel.*

of these categories conform perfectly with the theory: no object is more a *cup* than it is a *drinking vessel.* This is illustrated in the graph by the absence of points in the upper left diagonal. The absence of objects in the extreme lower right corner (*drinking vessels* but not *cups*) is due to my restricted selection of ceramic objects, which did not include drinking glasses, beverage bottles, and the like.

Figure 8 is the relation of *coffee cup* to *drinking vessel.* This graph is similar to the graph of Figure 7, and the lack of points in the lower right can be explained in the same way. *Drinking vessel* and *coffee cup* are related by inclusion (across two taxonomic levels), with *cup* linking them. If we compare Figure 8 with Figure 7, it appears that Figure 8 has been shifted down slightly. This downward shift in the values of *coffee cup* is not required by the accepted definition of inclusion of fuzzy sets, but this type of "much less than" relationship is suggested by the linking of these categories through two inclusion relations, each of which requires a less than or equal to relationship (that is, $\mu_{\text{coffee cup}}(x) \leq \mu_{\text{cup}}(x) \leq \mu_{\text{drinking vessel}}(x)$).

Figure 9 graphs *coffee cup* with *cup,* and again we see immediately that this is an inclusion relation, although a few objects have fallen above

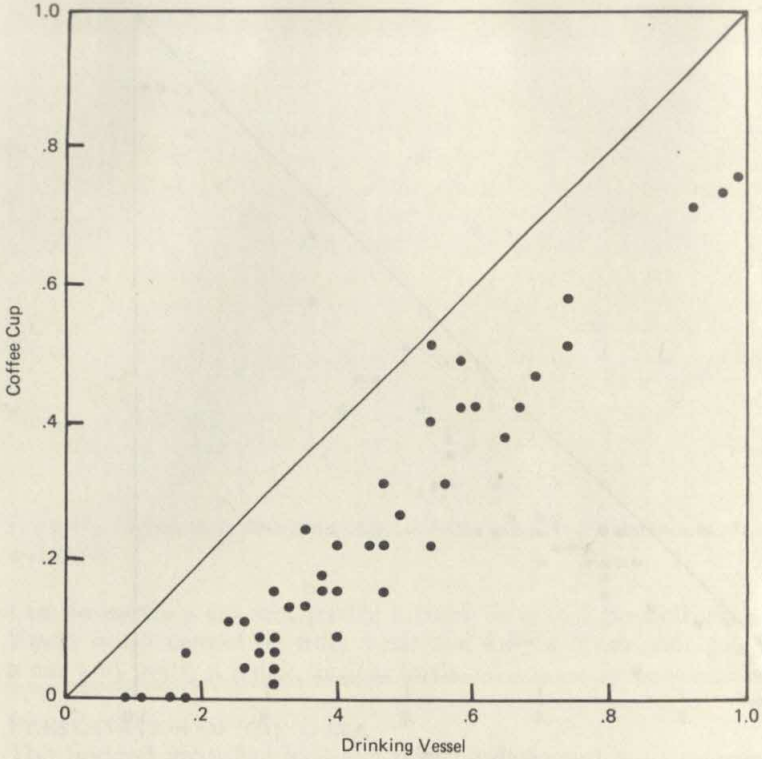

FIGURE 8. Elicited data showing that *coffee cup* is a subset of *drinking vessel.*

the diagonal. Objects above the diagonal in a graph of averages signify that at least some informants identified these objects as more *coffee cups* than *cups* (the disallowed direction of inequality). Although the magnitude of this deviation is probably small enough to be attributed to experimental error, we will see later that there is an interesting similarity between this relation and the inclusion of *mug* in *cup.*

In Figure 9 the lower right area is again empty, but in this case there is more than one plausible contributing factor. The empty lower right area is certainly due in part to the restriction of objects; I can easily imagine "good china tea cups" or "measuring cups" that informants might have judged not *coffee cups* but very much *cups;* these were not in my sample of objects. However, there is no reason to restrict our considerations to effects of visible features and informants' perception of form; we would intuitively expect function to play an important role in identification of *coffee cups.* Labov's (1973) findings indicate that if my informants made their judgments after coffee had been poured into one group of objects and tea into another, their amounts of membership in *coffee cup* would have changed considerably (this might also affect the membership of the objects in *cup*). If an object's membership in *coffee cup* is largely determined by use context, it is reasonable to suppose that when this

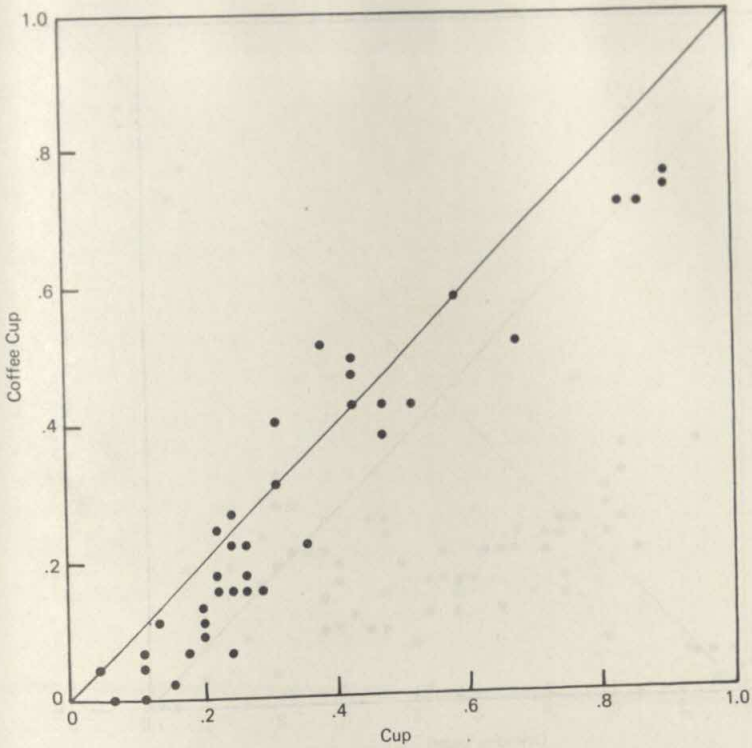

FIGURE 9. Elicited data showing that, with minor exceptions, *coffee cup* is a subset of *cup*.

context is undifferentiated the membership value in *coffee cup* would be precisely the same as the object's membership value in the superset *cup*. In this case, all points would fall on the diagonal.

In sum, the relations of set inclusion, or precedence, among the three categories *drinking vessel, cup,* and *coffee cup* occur essentially as predicted by fuzzy set theory. The data are consistent with the explanation that membership in *coffee cup* is partly influenced by the use context.

THE CONTRAST RELATIONS

This section considers relations among categories that are regarded as mutually exclusive in standard set theory, (that is, in Kay's formulation [1971:877], taxa in direct contrast or indirect contrast, but not inclusion contrast). Ten such contrasts were expected among the six categories used in the hedged interviews. Of these, all expectations except the contrast between *coffee cup* and *mug* occur. Three examples of contrast between fuzzy sets are sufficient to demonstrate the nine clear contrast relationships that occur in the data.

Figure 10 graphs the relation between *jar* and *drinking vessel;* as expected, all objects fall below and to the left of the diagonal. The absence

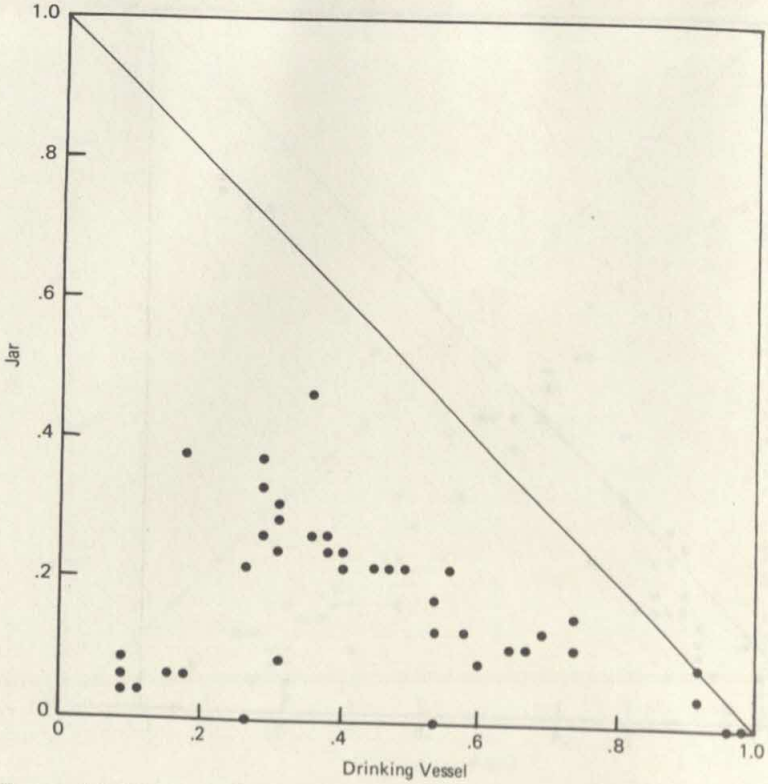

FIGURE 10. Elicited data showing that *jar* and *drinking vessel* are in contrast and that informants did not consider any objects very good members of *jar*.

of objects falling in the upper left part of this allowed area means that no objects were consistently ranked as good members of *jar*. Lack of typical jars was already evident in the small proportion of positive answers for *jar* (Table 2) and from informant statements during the interview (such as, "you don't have any good jars" and "none of these are jars because jars have to either have two handles or be made of glass"). As expected, all categories included in *drinking vessel* (*cup*, *mug*, and *coffee cup*) also exhibit a contrast relation with *jar*.

Figure 11 graphs the relation between *pitcher* and *drinking vessel*. This is clearly a contrast relation, although one errant object is a very typical drinking vessel while being a pitcher to a slight degree. One interesting aspect of Figure 11 is the cluster of eight objects[5] that are typical pitchers and are separate from the other objects. This suggests a sharply bounded (rather than fuzzy) category for pitcher. We cannot prove that the sharp boundary is cognitive, since distinctly shaped objects produced by the manufacturers (for example, a pronounced spout versus no spout) would also cause this discontinuity in informant judgments. It would be interesting to test whether this sharply bounded distinction is of cognitive origin or simply a byproduct of the shapes presented in

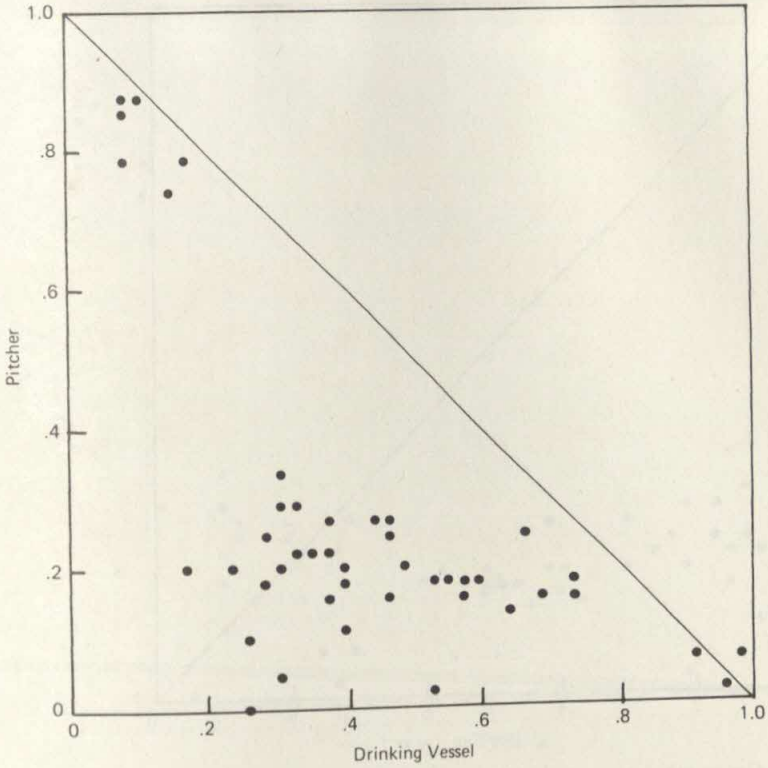

FIGURE 11. Contrast between *pitcher* and *drinking vessel* showing that informants made a sharp distinction between pitcher and nonpitcher.

the interview. But whatever the origin, note that fuzzy set theory does not prohibit categories with absolute boundaries; rather, they are one special case of all possible categories.

All subsets of *drinking vessel* are also in contrast with *pitcher* and exhibit the same type of sharply bounded distinction. This is illustrated in Figure 12 for the relationship between *pitcher* and *coffee cup*.

ARIGHT, YOUSE MUGS, WE KNOW YOU'RE IN THERE

Until this section, a neat correspondence between theoretical expectations and actual data has been achieved by excluding mention of the category *mug*. The contrasts of *mug* with *pitcher* and with *jar* occur as expected, as does the inclusion of *mug* in *drinking vessel*, but the relations of *mug* with *cup* and with *coffee cup* are not at all as expected.

Figure 13 shows a clear contrast relation between membership values in *pitcher* with membership in *mug*. *Jar* and *mug* also show a clear contrast relationship.

Figure 14 graphs the relation between *mug* and its superset *drinking vessel*. It meets the criteria for inclusion of fuzzy sets, as expected from the standard taxonomic interview, but has a curious shape. Objects that

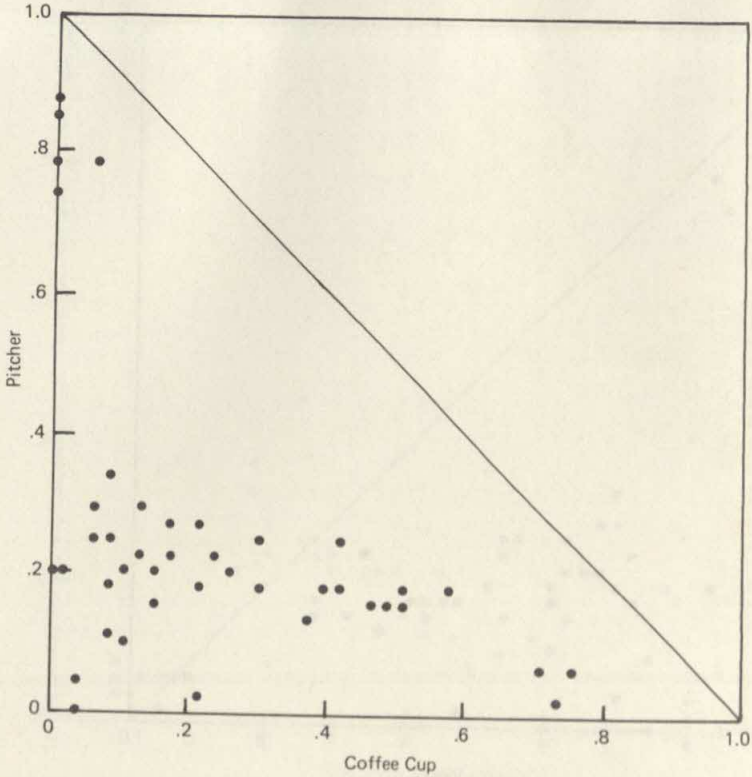

FIGURE 12. Contrast between *pitcher* and *coffee cup*.

are below .5 membership in *drinking vessel* behave exactly as would be expected in fuzzy inclusion; while for those between .5 and .75 membership in *drinking vessel,* their membership in *mug* is almost exactly the same as their membership in *drinking vessel;* and objects above .8 membership in *drinking vessel* (the "best examples of drinking vessels") show a drop of membership in *mug.* The shape of this curve is mentioned because it is different from the other inclusion relations; as in previous examples, empty areas under the diagonal of these graphs do not prove anything since the restricted selection of test objects will also cause empty regions.

In the standard taxonomic interview, the taxonomic relationship elicited most consistently from informants was the inclusion of *mug* in *cup.* However, the graph of values from the hedged identifications, presented in Figure 15, shows neither inclusion nor contrast. Twenty-five objects fall in the region disallowed for inclusion, while eleven fall in the region disallowed for contrast. To make matters worse, the points are distant from expected values.

Figure 16 illustrates the relationship between *mug* and *coffee cup.* From the standard taxonomic interviews, *mug* and *coffee cup* were ex-

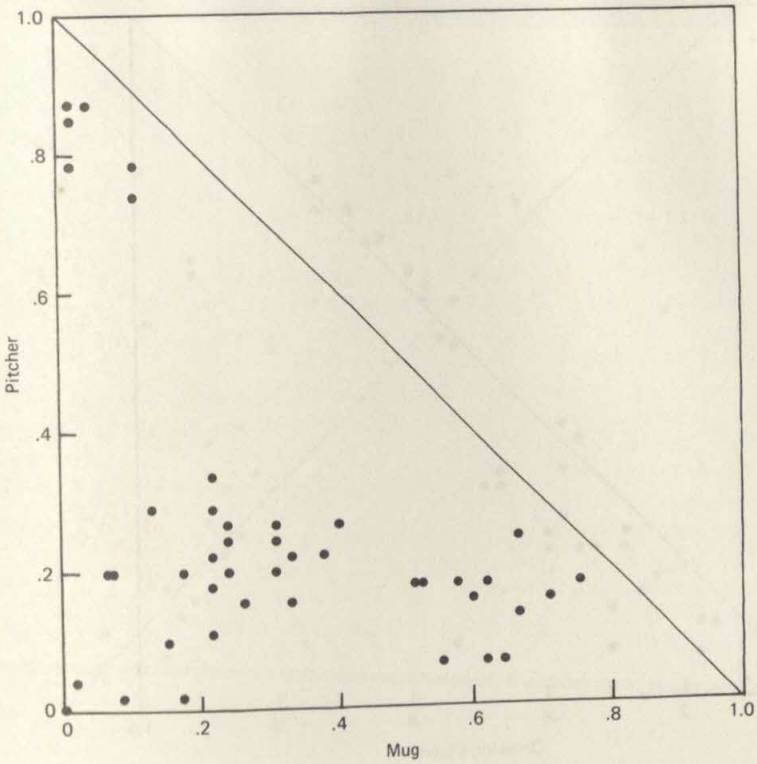

FIGURE 13. Contrast between *pitcher* and *mug*.

pected to be in direct contrast, since they were immediately included by *cup*. However, in the graph this relationship is neither contrast nor inclusion; numerous points fall outside the range of allowed values for either type of relationship. The graph of Figure 16 is quite similar to that of Figure 15, as would be expected by the close correspondence already noted between *cup* and *coffee cup*. Both of these curves show membership in *mug* increasing with (but at a slightly faster rate than) the category to which it is compared, until a critical value of membership in the other category is reached, then membership in *mug* drops sharply.

HOW CAN THESE ANOMALOUS RESULTS BE EXPLAINED?

Thirteen of the fifteen taxonomic relations (among the six categories investigated here) occurred essentially as expected from the standard taxonomic interviews. For two of these fifteen relations (between *mug* and its immediate superset and between *mug* and a category in direct contrast), the observed relationship differed considerably from expectations. Incorrect results for the relation between *cup* and *mug* were especially surprising since the same informants stated clearly and consistently during

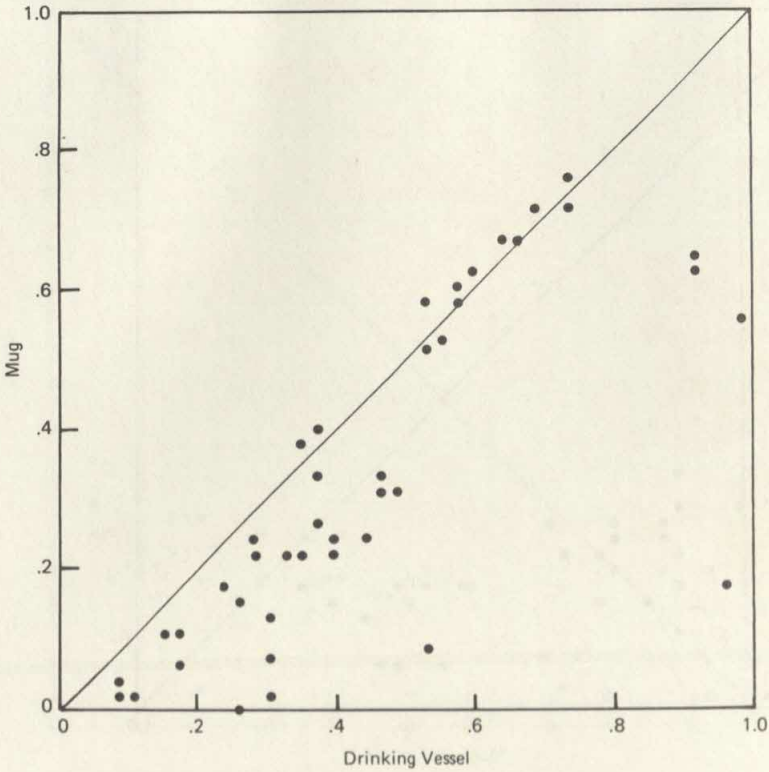

FIGURE 14. Inclusion of *mug* in *drinking vessel.*

the standard taxonomic interview that these categories were related by inclusion. Labov provides independent verification for this inclusion relationship in American college student English: "all informants agree that *cup* is a superordinate term for *mug:* that is, *mug* is a kind of *cup*" (1973:357). This problem is not restricted to these particular categories in this particular domain. Similarly problematic inclusion data have been reported from somewhat different interviews using hedges in Mexican Spanish (Kempton 1976).

This violation of the requirements of the fuzzy set theory definition of inclusion can be explained in three ways: (1) we can no longer believe informants when they tell us "an A is a kind of B," (2) there is something wrong with the measurement of membership grades used here, or (3) the fuzzy set definition of inclusion is wrong (or more accurately, it is not appropriate for the study of folk taxonomies). Since I believe that the data are essentially correct, I shall pursue the third of these possibilities. I suggest here an extension of fuzzy set inclusion consistent with these data and my own intuitions.

Fuzzy set theory predicts that when a fuzzy set A includes another fuzzy set B, the relationship $\mu_A(x) \geq \mu_B(x)$ will be true for all objects x. In

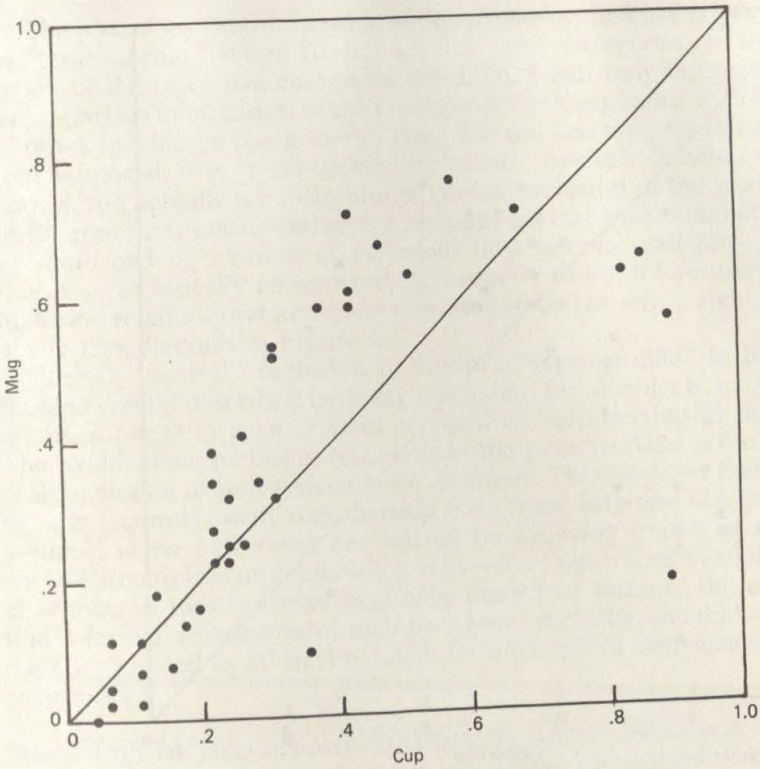

FIGURE 15. Elicited data on *mug* and *cup* showing that although informants consistently gave "mug" as a type of "cup," this relationship is far from meeting the expectations of fuzzy set inclusion.

fuzzy set theory *set inclusion itself is absolute;* it is defined as a relation that either holds or does not hold. However, if elements can be members of sets to a degree, we might ask if it could also be true that *sets are subsets of other sets to a degree.* In this vein, we might speak of the category *coffee cup* as being "typically included" in the category *cup* at an "inclusion value" of .95, while *mug* would be included in *cup* at an inclusion value of only .75. Given this sort of graded relationship between categories, an object that was a very typical *coffee cup* would necessarily be a very typical *cup,* but an object that was a very typical *mug* would not be a very typical *cup.*[6] Nevertheless, given such circumstances informants would surely describe both *coffee cup* and *mug* as "kinds of cups."

Reference to Figure 15 reveals that this explanation is consistent with the observed distribution of membership values of *cup* and *mug.* A parallel argument can be constructed for contrasting categories such as *mug* and *coffee cup.* Note that a hypothesis of grades of inclusion is logically independent of a hypothesis of grades of membership as proposed by fuzzy set theory. We could, for example, have a theory that required absolute

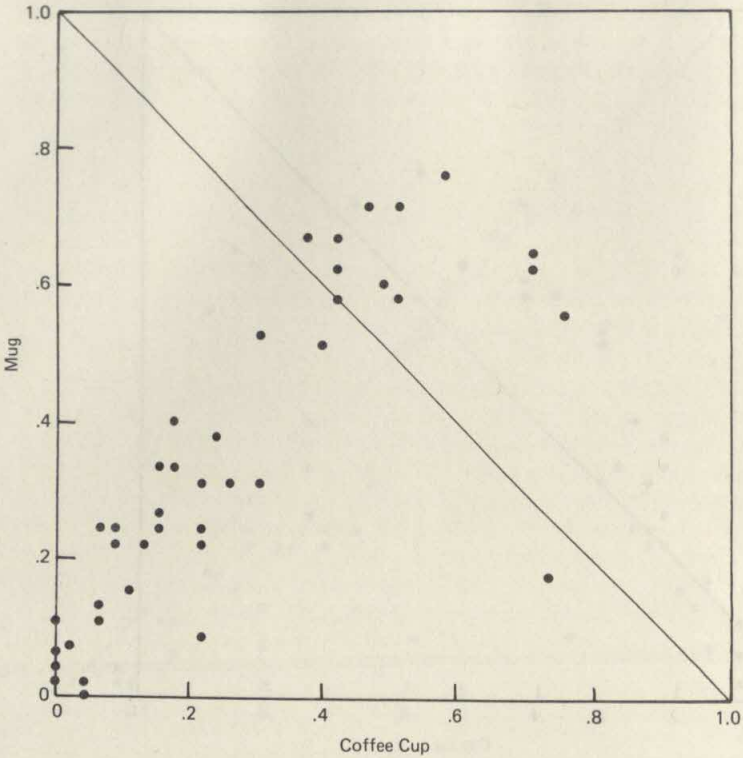

FIGURE 16. Elicited data on *mug* and *coffee cup* showing these to be far from the expected contrast relationship.

category membership but provided for gradations of relations among categories. I am proposing that the data require an adequate theory of human cognition to include both.[7]

The idea that the inclusion of one set in another occurs to varying degrees will seem foreign to some, and this suggestion has not been universally well received. I argue below that differing grades of inclusion are implicit in the work of others, specifically in Rosch's discoveries about semantic categories and Berlin's discussion of "type-specifics."

Rosch measures responses to sentences like "A chicken is a bird" and concludes that, for example, *robin* is a "central member" of *bird* while *chicken* is a "peripheral member"of *bird* (1973:130–140). But *chicken* and *robin* are labels for categories: they are not *members* in the normal sense that this word is used in set theory (although many specific living organisms are members of both *chicken* and of *bird*). Thus, Rosch is not comparing membership of elements in categories, but inclusion between pairs of categories. Rather than central and peripheral *members*, these might more correctly be called central inclusion and peripheral inclusion.

The idea of gradations in set inclusion is also implicit in Berlin's concept of "type-specific." When Berlin says that some categories are the "ideal type" of their generic categories (1972:59), I can only interpret this as a comparison of inclusion of the type-specific in the generic with inclusion of other specifics in the generic. Thus, it is not the type-specific category that is special, it is "type-specific inclusion." Berlin's dichotomy is here considered actually a continuum of grades; inclusion of the type-specific in its generic is not considered a separate special inclusion, but just the extreme end of a range of inclusions in a generic category (and one that may be lexically recognized). It is exactly these differences between inclusion relations that are being represented by the left-to-right ordering of my tree diagram in Figure 1.

Rosch's "central" inclusion or Berlin's "type-specific" inclusion are those correctly described by fuzzy set theory; for all objects, membership in the superset is greater than or equal to membership in the subset. The problematic inclusion (*cup-mug* in the present data) is the "peripheral" inclusion or non-type-specific inclusion. This has been documented by one example, with membership data from fifty-one objects. But in general, when categories are related by differing grades of inclusion, we lack a complete understanding of the effect upon relative membership of objects in those categories. I have described some of the properties that a formal description of such peripheral inclusion should have. More data are needed to fill in the details for a complete formalization of this relationship.

Similarity of Hedges and Statements About Inclusion

In cases like the inclusion of *mug* in *cup*, I have just argued that inclusion is a graded relation. It now becomes pertinent to note the similarity in English between statements about inclusion and statements about intermediate grades of membership. Although standard nonacademic English does not use the words *inclusion* and *subset*, there are three common means of expressing these relations:

1.*An A is a type of B.
2. An A is a kind of B.
3. An A is a sort of B.

An interesting change takes place when the article (a) of these statements is moved:

1. An A is type of a B.
2. An A is sort of a B.
3. An A is kind of a B.

Although (1) is nonsense, (2) and (3) are common hedged statements denoting an intermediate grade of membership. When the article appears in both positions, we have:

1. An A is a type of a B.
2. An A is a kind of a B.
3. An A is a sort of a B.

By my own intuitions, (1) still denotes a subset relation, while (2) and (3) are ambiguous; they could denote either membership gradations or subsets—or perhaps a subset gradation.

SUMMARY

Data from standard taxonomic interviews were compared with data elicited by using hedged statements to identify objects. In thirteen of the fifteen relations between categories, informant statements of inclusion and contrast corresponded well with the hedged identifications according to the predictions of fuzzy set theory. However, for two relationships the informant statements "A mug is a kind of a cup" and "Mugs and coffee cups are different kinds of cups" were quite inconsistent with these informants' identifications of objects. This inconsistency leads to the hypothesis of differences between different instances of the inclusion relation itself: in this specific case, the category *coffee cup* has a greater inclusion value in *cup* than does the category *mug*. Such gradations in inclusion are shown to be implicit in the work of Berlin and Rosch. Standard taxonomic theory does not even offer any means of measuring these differences among relationships, much less provide a possible explanation.

Surely, fuzzy set theory captures an important intuition about the way we think; many of our categories do seem to have gradations rather than being internally homogeneous and sharply bounded. But if this theory is to be useful to cognitive anthropology its definitions must be extended to allow for not only grades of membership of an object in a category, but also for the grading of relationships between categories. Extended in this manner, fuzzy set theory offers a powerful formal device for the understanding of human cognition.

POSTSCRIPT

Since the original publication of this paper, several explanations of its data have been advanced. None are quite adequate: Kronenfeld (1978) and Hunn (1979) seem to be headed in the right direction but are too vague, while Smithson (1978) succeeds in the simpler task of measuring deviation from fuzzy set inclusion, without *explaining* it. Zadeh suggests that normalization of the superset would solve the problem (1978:415), but even if this proposal were theoretically motivated, it does not explain the data in Figure 15. In a thorough discussion, Sailer (1978) points out that the present article: (1) is one of the few applications of fuzzy set theory to real data, (2) uses a measurement that is not really based on a fuzzy set, but on the finite lattice {0, .2, .4, .6, .8, 1.0}, and (3) demonstrates only that the data are fuzzy, without proving the cognitive process producing that data to be fuzzy. More complete data, relating feature values to membership in *cup* and *mug*, can be found in Kempton (1979).

NOTES

1. I gratefully acknowledge helpful criticism on this material from Cecil Brown, Arthur Friedman, Carl Gregory, Carol Rinnert, David Robichaux, Mark Ross, Sue Schmerling, Doug R. White, and Dorothy D. Wills; particularly detailed

comments were provided by Brian Stross, Ann Millard, Paul Kay, and Ben Blount. Responsibility for errors remains my own.

This work was conducted during my graduate study at the University of Texas at Austin. Funding for computer resources was provided by The National Science Foundation under doctoral dissertation research grant SOC 75–20505. The programming language Pascal and the statistical package SPSS were a considerable help in the analysis; I thank their authors and implementors.

2. Either *coffee cup* or *tea cup* was (in all but one case) listed before *mug*, although *mug* was listed more frequently. For this reason, *mug* is placed to the right of the other two in Figure 1. It seems likely that the slightly lower frequency of naming *coffee cup* and *tea cup* was due to my informants' considering these as simply *cups* (that is, for some informants one or the other of these specific categories had the same name as the generic catetory).
3. Ten additional interviews have been carried out using sixty objects, and the results are strikingly consistent with the results presented in this paper.
4. Derivation of direct contrast between fuzzy sets is as follows: in standard taxonomic theory, two taxa A and B are in direct contrast if they are both immediately included by a third taxon C (Kay 1971:877); the contrasting taxa A and B have no member in common (1971:870), thus B is a subset of the complement of A (denoted A'). From fuzzy set theory, we know that if B is a subset of A' then $\mu_B(x) \leq \mu_{A'}(x)$, and if A' is the complement of A then $\mu_{A'}(x) = 1 - \mu_A(x)$ (Zadeh 1965:340, Lakoff 1973:461), so by simple substitution $\mu_B(x) \leq 1 - \mu_A(x)$ or $\mu_A(x) + \mu_B(x) \leq 1$.

   Unfortunately, the extension of the notion of direct contrast (or mutual exclusion) to fuzzy sets is not a resolved issue. The derivation presented here cannot be regarded as a proof, because it combines theorems from standard set theory with those from fuzzy set theory. A contrast relationship derived from a fuzzy complement (such as the one presented here) implies a mutual exclusion and a partition with properties between those of Kay and McDaniel's strong fuzzy partition and their "weakened" fuzzy partition (1978:641–644).
5. Only six distinct points are visible in Figure 11 because three points are plotted in exactly the same place.
6. Readers familiar with fuzzy set theory will recognize that an explanation of this sort implies that fuzzy set union is not to be used in partitioning sets. Union is defined as follows: if fuzzy set A is the union of fuzzy sets B and C, the membership value of any element in A is equal to either its membership in B or its membership in C, whichever of these two membership values is greater (see Zadeh 1965, 1973; Lakoff 1973).

   The only evidence I offer that the standard formulation of fuzzy set union is incorrect is my intuition about the above explanation of *cup* and *mug*. In my proposed explanation, *cup* absolutely includes *coffee cup* and fuzzily includes *mug*, so that if an object that otherwise would have been a very typical *mug* was less of a *mug* by virtue of its having some *coffee cup*ish features, then I would expect this object to be more a *cup* than it was either a *mug* or a *coffee cup*. In other words, an object that is between being a *mug* and being a *coffee cup* is still very much a *cup*.

   An informant confronted with such an object and asked "Is this a mug or a coffee cup?" might respond with something like the following: "Well, I guess it's a mug [short pause] but in a way it is also a coffee cup [longer pause] it's definitely a cup of some sort." This situation would correspond to membership values for this object x of something like:

   $\mu_{mug}(x) = .75$
   $\mu_{coffee\ cup}(x) = .40$
   $\mu_{cup}(x) = .85$

In this example, drawn purely from my own intuitions, the accepted definition of union of fuzzy sets, that is,

$$\mu_{cup}(x)=\text{maximum } [\mu_{mug}(x), \mu_{coffee\ cup}(x)]$$

does not correctly describe partition.

I agree with Kay and McDaniel that niether the product nor the sum of membership values is appropriate for defining union of fuzzy sets (1975:14), but sum, product, and maximum are certainly not the only arithmetic operations available for this definition. I believe union of fuzzy sets is closer to an "interactive or" (Zadeh 1976:288):

$$\mu_{A<\text{or}>B}(x)=(\mu_A(x)+\mu_B(x))-(\mu_A(x)\mu_B(x))$$

Under such a partially summing union definition, low or moderate membership in several subsets implies high membership in the superset.

A summing union definition for fuzzy sets is parallel in standard set theory to allowing an incomplete partition of the superset. In the accepted definition of taxonomic structures, supersets are completely partitioned (as in Kay 1971:869), but Hunn argues that even folk biological classification systems are frequently incompletely partitioned (1976:510–511).

7. In response to similarly problematic data presented in Kempton (1976) Zadeh has suggested that in general, any nonfuzzy relation (such as inclusion) can be "fuzzified." Under this convention, the degree of inclusion is a function of the degree to which the criteria of the definition are met. But he considers such a "fuzzification" of the subset relation to be "application dependent and not susceptible of definition in a standard form" (letter of 28 January 1977). However, if fuzzy set theory is intended to provide a means of formally describing human cognition, and if the problems discovered here with *cup* and *mug* occur frequently, then the definition needs to be modified, not just loosened.

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11

The Semantics of Kin Term Usage: Transferred and Indirect Metaphorical Meanings[1]

RONALD W. CASSON*†

Critics have argued that, while practitioners of formal semantic analysis have been able to specify the genealogically-defined meaning of kinship terms, they have not been able to account for the social meaning communicated by the actual use of these terms in context. As D'Andrade (1970:131) has put it, these critics typically cite as evidence for this objection instances of usage they observed in the field, e.g., "Old Joe refuses to call one of his genealogical cousins by the proper 'cousin' term, while calling young Bill, to whom he is not genealogically related at all, by this same 'cousin' term." Because kin terms are thus seen to be used both genealogically and non-genealogically, they conclude that a formal analysis based upon genealogical connections cannot adequately or accurately state the meaning of kinship terms. This criticism has been termed the "usage objection" to formal analysis (D'Andrade 1970:130–131).

An attempt is made here to come to grips with the usage objection. The approach adopted here in order to do so assumes that "social relationships act as intervening variables between linguistic structures and their realization in speech" (Blom and Gumperz 1972:409). The analysis presented in this paper is aimed at specifying the meaning communicated

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† Source: *American Ethnologist* 2:229–238 (extensively revised).

in the socially situated usage of kinship terms in a Turkish village, both the literal genealogically-defined meaning and the metaphorical (non-genealogically-defined) meaning of the terms. An attempt is made to show that there are two varieties of metaphorical meaning conveyed in the use of these terms: transferred metaphorical meaning and indirect metaphorical meaning.

SOCIOLINGUISTICS AND SPEECH ACT THEORY

The approach outlined here is an ethnosemantic approach based upon the results of research in two fields of study: sociolinguistics and speech act theory. Both of these areas of research are concerned with achieving an understanding of the use of language in social contexts. Sociolinguists studying "communicative competence" in the area of sociolinguistics known as the "ethnography of speaking" (cf. Hymes 1962; 1972) have been concerned with the contextual use of second person pronouns, alternate verb inflections, systems of honorifics, titles, greetings, kinship terms, alternate style levels, dialects, languages and silence or non-speaking (cf. Brown and Gilman 1960; Friedrich 1966; Ervin-Tripp 1972; Brown and Ford 1961; Fischer 1964; Tyler 1969; Geoghegan 1971; Labov 1966; Blom and Gumperz 1972; Rubin 1968; Gumperz and Wilson 1971 and Basso 1970). To illustrate this approach, we can look at pronoun usage. Sets of second person pronoun alternates, for example, have been shown to be used as follows: in French, Russian, and Turkish (as well as in a number of other languages), the second person singular pronoun (*tu, ty, sen,* usually abbreviated T) is used by the speaker in addressing lower status or intimate hearers, and the second person plural pronoun (*vous, vy, siz,* abbreviated V) is used in addressing higher status or non-intimate hearers. The meaning communicated by the usage of these sets of alternates has been defined in terms of features on two semantic dimensions: the power dimension and the solidarity dimension. The power dimension is asymmetrical: higher status participants use T to lower status participants and receive V in return. The solidarity dimension is symmetrical: participants of equal status exchange either mutual T if they are intimate or mutual V if they are non-intimate (Brown and Gilman 1960:258–259).

The concept "speech act" originated in the work of the philosopher J. L. Austin (1962), whose principal concern was to argue that the uttering of a sentence involves not only saying something but also doing something. Searle (1969), building upon Austin's work and revising it somewhat, argues that in speaking a sentence a speaker utters certain words and morphemes, expresses a certain semantic content, and performs a particular act or activity, such as stating, commanding, promising, or wishing (Searle 1969:23–24). The distinction between the semantic content expressed and the illocutionary act or (for simplicity sake) speech act performed in uttering a word or string of words may be seen in the following sentences.

1. Harvey ran in the marathon.
2. Did Harvey run in the marathon?
3. Harvey, run in the marathon!

Utterances of these sentences by a speaker to a hearer in appropriate contexts normally constitute different speech acts: (1) is characteristically an assertion or statement, (2) a question, and (3) a request or command, but all three sentences have a common semantic content. In all three, the speaker refers to a particular person, Harvey, and to a particular event, a marathon, and predicates a relation between these two referent entities—the act of one "running in" the other. The three sentences all express this semantic content, but differ from each other in that (1) asserts it, (2) questions it, and (3) orders it.

Utterances, then, both communicate a certain semantic content and indicate a certain function or force—the speech act. In English, the function or force of a sentence may be indicated overtly by a performative verb (e.g., order, promise, warn, wish, etc.) or may be understood from stress, intonation, word order or contextual factors. The distinction here is between sentences such as

1. I order you to go.
2. I promise that I will come.
3. I state that he did not do it.

which Austin termed explicit performatives, and sentences which he termed implicit performatives, such as

4. Go!
5. I will come.
6. He did not do it.

Austin argued that explicit and implicit performative sentences are used in accomplishing the same speech acts. Utterances of sentences (1) and (4), for example, are both commands or orders to the hearer to go (Austin 1962:32), (2) and (5) are both promises on the part of the speaker that he will come at some future time (Austin 1962:69), and (3) and (6) are both statements by the speaker that some third party did not do something (Austin 1962:133).

The notion "speech act" has recently been introduced into linguistic theory as the "performative analysis" by Ross (1970) and as the "theory of hypersentences" by Sadock (1969). These two schemes are, for all practical purposes, equivalent. Ross and Sadock both argue that it is universally the case that the highest sentence node in the abstract structure underlying any sentence has the following major constituents: (1) a subject noun phrase referring to the speaker of the sentence; (2) an indirect object noun phrase referring to the hearer (or hearers) of the sentence; (3) a performative verb indicating sentence type (i.e., interrogative, imperative, declarative); and (4) an embedded sentence functioning as the complement of the verb (Sadock 1969:342). Sadock adopts the term "hypersentence" to refer to this highest sentence node and maintains that a hypersentence must dominate the rest to the abstract structure of any sentence (1969:307).

In his discussion of hypersentences, Sadock points out that when second person pronoun usage is determined by status, it is determined not by the status of the hearer alone, but by the status of the hearer relative

to the speaker, even when no explicit reference is made to the speaker in the surface sentence uttered by the speaker (1969:296). Likewise, when second person pronoun usage is determined by intimacy, it is determined by the intimacy relationship between the speaker and the hearer, whether or not there is overt reference to the speaker in the utterance. Second person pronoun usage is thus determined by the social relations holding between classes or categories of speakers and hearers. A social relationship in which (the category of) the speaker is of higher status than (that of) the hearer defines a context in which T is appropriate; one in which the speaker is of lower status defines a context in which V is appropriate. A social relationship in which (the categories of) the speaker and the hearer are close or intimate defines a context in which T is appropriate; one in which they are distant or non-intimate defines a context in which V is appropriate. Speech act theory, in the extended formulations of Sadock and Ross, posits an abstract structure (a hypersentence) underlying all sentences which makes reference to speech act participants, thus accounting for the fact that second person pronoun usage is governed by social relations between speakers and hearers, even though this reference to participants may be only implicit in the sentence that is actually uttered.

KIN TERM USAGE IN A TURKISH VILLAGE

It may now be shown that the above description of research in sociolinguistics and speech act theory provides a means of overcoming the usage objection to formal analysis. This may be demonstrated by an analysis of kin term usage data collected in Yeniyurt (pseudonym), a Central Anatolian village (population approximately 250) located some 30 kilometers from Ankara, the capital of Turkey.

The kinship terminology of Yeniyurt was investigated as part of a larger study of the social organization of the village currently being conducted by Dr. Bânû Özertuğ and the author. Several techniques were employed in collecting data on the terminology and its use. First, genealogies were collected until the investigators knew the relationship between every pair of individuals in the village and the kin term reciprocals used in each case to label this relationship. All genealogy-collecting sessions were recorded on tape.

Secondly, the investigators observed and recorded the use of kin terms in natural interactions in the village. These observational data were collected primarily by Özertuğ, who is a native speaker of Turkish. In recording the usage of kin terms between particular interlocutors related in terms of particular genealogical ties, Özertuğ was further aided by the fact that her acquaintance with the village and the villagers dates from 1965 (cf. Özertuğ 1966).

A third technique used in studying the usage of kin terms was the tape recording of many hours of natural conversation among villagers about kin related matters. These conversations were "natural" despite the unconcealed presence of a tape recorder and despite the fact that they were in most cases initiated and occasionally re-directed by the

investigators. While the tape recorder did occasionally tend to inhibit conversation in its initial stages, its presence in nearly all cases was quickly forgotten and in no way constrained subsequent verbal interaction (Blom and Gumperz 1972 also found this to be the case in their work in a Norwegian village). The questions of the investigators were likewise only a slight deterrent to the naturalness of these conversations. Once discussions were initiated, the investigators participated as little as possible.

The importance of these recorded conversations cannot be overstated. The locally significant phrases used in expressing kin relationships were discovered by repeatedly listening to these tapes; in this way, two questions were found which determined two contextual situations of kin term usage. The first of these is a situation in which the informant uses a term to a third person in the absence of the designated kinsman. This usage is elicited by the question, *"Sana ne düşer?"* ('Into what [kin category] does he/she [the particular alter] fall?'). The second is a situation in which the informant is asked to report the term he uses in face-to-face conversation with the designated kinsman. The question used in eliciting this usage is, *"Yüzüne ne dersin?"* ('What do you say to his/her face?'). The names of all the married villagers were written out on 2 x 4 cards, and twenty-eight informants (fifteen males and thirteen females) were asked the local questions about the usage of kin terms between themselves and the alters listed on the cards.[2]

The data elicited in this manner are regarded here as evidence of the actual use of kin terms between real Egos and alters. Responses to the local questions are reports of actual usage in particular situations. Informants were not only able to produce such reports, they were also able to do so with no difficulty. It may be argued that data elicited in this way are of the same logical status as the data analyzed in second person pronoun studies (e.g., Brown and Gilman 1960). In both cases, the data are the verbal reports of informants as to how they actually use the term of a lexical system. The reliability of these data is attested to by the great success of the pronoun studies. In addition, it is supported by the argument that informants are more likely to be able to report kin term usage accurately than pronoun usage. This follows from the presumption that, because pronouns are grammatical elements (function words) whereas kin terms are lexical elements (content words), informants are more aware of how they use kin terms than of how they use pronouns (cf. Lyons 1968:235; Friedrich 1966:217–220). Conclusive evidence for the reliability of these elicited data is provided by the observational data (i.e., data collected by the second procedure). These independently collected data are direct evidence of usage and serve as a check on the elicited data. It is, in my opinion, an important empirical finding that the observational data and the elicited data are nearly perfectly matched.

TRANSFERRED METAPHORICAL MEANING

For the sake of brevity, only address usage, that is, only face-to-face use in speaking directly to a hearer, will be described here. This set of terms includes kin terms, FN (first name), and kin terms prefaced by FN (e.g.,

Osman *dayí*).[3] The set of terms used in address to kinsmen includes the following: *dede* (PaFa), *ebe* (PaMo), *baba* (Fa), *ana* (Mo), *apça* (FaBr), *dayí* (MoBr), *hala* (FaSi), *diaza* (MoSi), *enişte* (SiHu), *nene* (BrWi), *aga* (eBr), *aba* (eSi), FN*aga* (PaSiSo), FN*aba* (PaSiDa), and FN (all younger alters). (Full ranges and extension rules are given in Casson 1973 and 1978.) The basic meanings of these terms can be componentially defined in terms of the following dimensions and features: K, kinsman, which takes the values K_1 consanguine and K_2 affine; L, laterality, which takes the values L_1 lineal and L_2 collateral; C, sex of intervening relative, which takes the values C_1 parallel and C_2 cross; G, relative generation, which takes the values G_0 same generation as Ego, G_{+1} one generation superior to Ego, G_{-1} one generation inferior to Ego, etc.; A, relative age, which takes the values A_1 relative older than Ego and A_2 relative younger than Ego; and R, sex of relative, which takes the values R_1 male and R_2 female. These paradigmatic relations are shown in Figure 1.

Subsets of these terms are used in addressing nonkin as well. In this context, the meanings conveyed by the terms are derived metaphorically from their kin address senses. Address usage to nonkin Yeniyurt covillagers and other "village" people differs from usage to nonkin "city" outsiders. The terms used to nonkin villagers are FN*dede*, FN*dayí*, FN*hala*, FN*aga*, FN*aba*, and FN (cf. Figure 2). The terms used in address to nonkin city people, e.g., shopkeepers, police, and the like, are *dede, amca, teyze,*

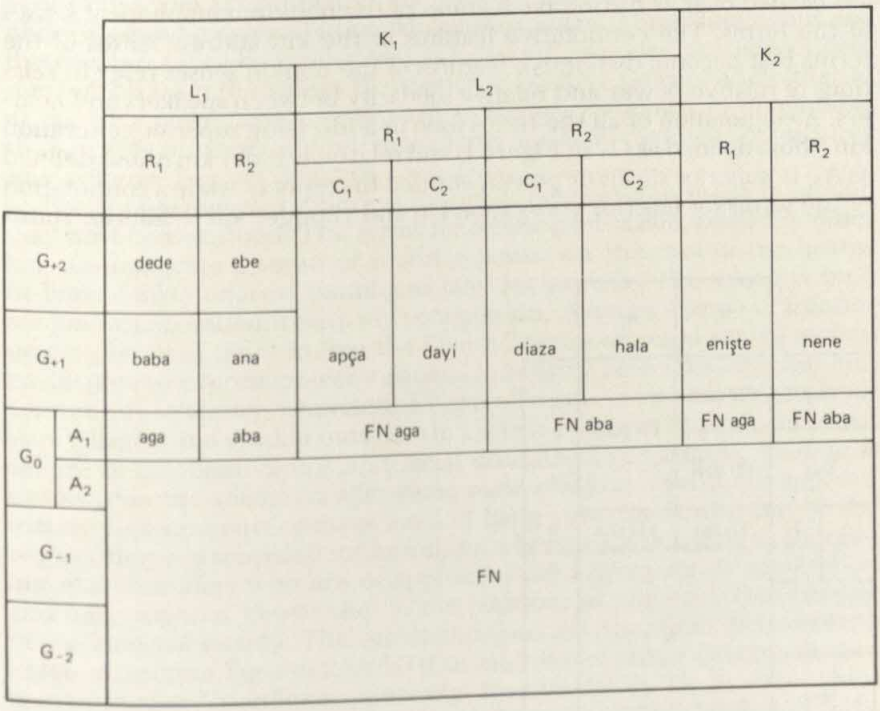

FIGURE 1. Kin address paradigm.

ağabey, abla and FN (cf. Figure 3).[4] Usage of nonkin address terms within Yeniyurt always occurs preceded by FN, but outside the village it occurs with and without FN, depending upon whether or not Ego knows alter's first name. The terms *amca, teyze, ağabey* and *abla* are city or Standard Turkish alternatives for *apça, diaza, aga* and *aba*.

The meanings conveyed in the nonkin usages of these terms are simple metaphoric transfers from the kin address senses of the terms. In metaphoric transfers, aspects of the primary or more basic referential meaning of a term are transferred to another referent which is similar in some way to the more basic referent. In this semantic transfer, some distinctive features of the basic sense of the term are neutralized and some connotative features become distinctive, so the distinctive features of the transferred metaphorical sense of the term are a combination of features shared with and carried over from the basic sense of the term and features that are only connoted by the basic sense of the term.

The transferred metaphorical senses of the nonkin address terms represented in Figures 2 and 3 are derived from the kin address senses of the terms represented in Figure 1. In the metaphoric transfer of the kin address senses, features on the dimensions K, L, C and A are neutralized and transferred only as aspects of connotative meaning in nonkin address usage. Only features on the dimensions R (sex of relative) and G (relative generation, which now takes the values G_0 approximately the same generation, G_{+1} approximately Ego's parents' generation, etc.) are carried over as distinctive features of the nonkin metaphorical senses of the terms. The connotative features of the kin address senses of the terms that become distinctive features of the nonkin senses refer to relations of relative power and relative solidarity between speakers and hearers. A connotation of all the terms used in addressing superior generation kin (those defined as G_+ in Figure 1) and relatively older kin (those defined as A_1) is 'superior status or power, entitled to respect,' while a connotation of the terms for inferior generation kin and younger kin is 'inferior status

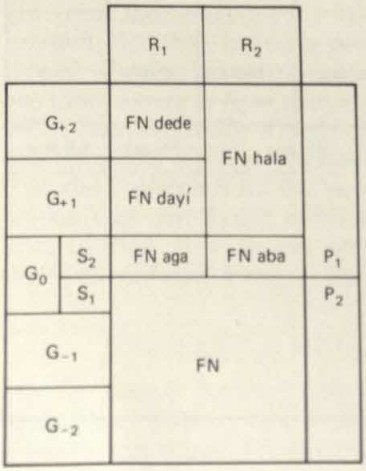

FIGURE 2. Nonkin "village" address paradigm.

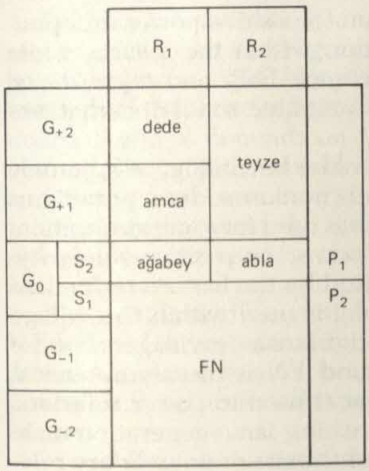

FIGURE 3. Nonkin "city" address paradigm.

or power, not entitled to respect.' Connotations of the terms used in addressing same generation kin (those defined as G_0) are 'mutual intimacy or solidarity' and 'mutual nonintimacy or nonsolidarity.' Power relations are described by informants in terms of *saygí* ('respect, deference') and solidarity relations in terms of *samimiyet* ('sincerity, intimacy'). Power and solidarity are thus the dimensions which, together with the carried-over dimensions sex of relative and relative generation, define the metaphorical senses conveyed in nonkin address usage. These dimensions and their features may be symbolized as follows: P, a power or status dimension, which takes the values P_1 superior or positive power and P_2 inferior or negative power and S, a solidarity or intimacy dimension, which takes the values S_1 intimate or positive solidarity and S_2 nonintimate or negative solidarity (cf. Figures 2 and 3).

The transferred metaphorical meanings conveyed in usage to nonkin may now be described. The terms for senior generation, relatively older kin communicate a sense of superior power on the part of the hearer in both nonkin address paradigms (and reciprocally FN, which is used for junior generation, relatively younger kin, conveys a sense of inferior power), but they differ in that the Figure 2 terms are used among nonkin covillagers to express power relations in a spirit of cooperativeness and community solidarity, whereas the Figure 3 terms are used between Yeni-yurt villagers and nonkin outsiders to express power relations in an atmosphere of competitiveness and social distance. For example, FN*dayí* is used within the village in addressing male villagers who are of approximately Ego's parents' generation and have superior power due to the respect they are accorded in the village, whereas *amca* is used in addressing male outsiders who are of approximately Ego's parents' generation and have superior power due to the positions of authority they occupy in the external society. The essential difference, therefore, between the usage of terms in Figures 2 and 3 is, as informants stated, that the former convey greater "friendliness, sincerity" than the latter. This is also evident in the fact that it is the cross-collateral G_{+1} terms (*dayí:* MoBr and *hala:*

FaSi), which in their kin address senses connote positive power and positive solidarity, that are transferred to nonkin within the village, while it is the parallel-collateral G_{+1} terms (*amca/apça:* FaBr and *teyze/diaza:* MoSi), which connote positive power and negative solidarity, that are transferred to nonkin city outsiders.

The terms for same-generation, relatively older kin (in Figure 1) convey positive power or negative solidarity in both nonkin address paradigms (Figures 2 and 3), and reciprocally FN, which is used for same generation, relatively younger kin, conveys negative power or positive solidarity. These terms all have two senses, as is indicated by the feature redundancies in Figures 2 and 3. FN*aga,* for example, is used within the village in addressing approximately same-generation male covillagers whose power is superior to that of the speaker, and FN is the asymmetrical reciprocal of this power sense of FN*aga* that is used to power inferiors. FN*aga* is also used within the village in addressing same-generation male covillagers with whom the speaker has a nonintimate or nonsolidary relationship (a competitor or rival), and FN is also used in addressing same-generation male covillagers with whom the speaker has an intimate or solidary relationship (a friend or ally). The usage of *ağabey* and FN between Yeniyurt villagers and outsiders is much the same, but it conveys a lack of community solidarity, in contrast to the use of FN*aga* and FN which indicates solidarity.

Earlier, it was pointed out in discussing speech act theory that the social relationship between a speaker of one category and a hearer of another determines the meaning conveyed in second person pronoun usage, even when no overt reference is made to the speaker. Since the metaphorical meaning communicated in the use of the nonkin address terms also depends on the speaker-hearer relationship, it is crucial that this context of usage be thoroughly examined. The kin address terms (Figure 1 terms), like personal pronouns, are referential indexes, since they are used to accomplish both referential and indexical functions. They are "duplex signs" or "shifters," functioning simultaneously at two levels: the level of semantic or referential meaning and the level of pragmatic or indexical meaning (cf. Jakobson 1957; Bean 1975; Silverstein 1976). The meaning conveyed in the use of personal pronouns involves both reference to categories of persons distinguished by person (first, second, third) and number (singular, plural) and indexing of participants in the speaker and hearer roles. The referential meaning of a shifter depends on the indexing of participants in contexts of usage, and it is in this fact that the appropriateness of the term "shifter" is apparent. The meaning of personal pronouns shifts as speakers shift; the referents of *I* and *you,* for instance, can be determined only with respect to the indexing of the speaker *(I)* and the hearer *(you)* in actual situated usage. The meaning conveyed in the use of kin address terms is contextually situated in the same way: the terms both refer to kin categories distinguished by the dimensions and features illustrated in Figure 1 (e.g., K kin status, L laterality, C cross-parallel, G relative generation, etc.) and index kinsmen of the speaker as the referents denoted by the terms in situations of usage.

The nonkin address terms which convey transferred metaphorical senses in Yeniyurt (the Figure 2 terms) also function as referential indexes, but in addition—again like personal pronouns—they also function as pure indexes (cf. Silverstein 1976:31). The meaning of the second person pronouns T and V depends on both identifying the hearer as the referent of the pronoun in the situation of usage (the referential-indexical function) and the nonreferential or pure indexing of the power and/or solidarity relationship between the speaker and the hearer (the pure deferential indexical function and/or the pure solidarity indexical function). The nonkin address terms are also used to accomplish both referential-indexical and pure indexical functions: the transferred metaphorical senses of the terms involve both identifying or classifying the hearer as the referent of the term in the situation of usage and the pure indexing of relative power and relative solidarity relations between the speaker and the hearer.

These separate functions of the nonkin address terms are represented by features on different dimensions in the componential definitions of their transferred metaphorical senses. For brevity sake, only the terms used in addressing male nonkin in the village will be illustrated. FN*dayí* is used in Yeniyurt to convey the metaphoric sense R_1 (male), G_{+1} (approximately the speaker's parent's generation), and P_1 (positive power), in opposition to FN which reciprocally indicates the sense R (neutral sex of relative), G_{-1} (approximately the speaker's child's generation) and P_2 (negative power). FN*dede* is used to communicate the metaphoric sense R_1, G_{+2} and P_1, in opposition to FN which reciprocally indicates the sense R, G_{-2} and P_2. FN*aga* is used to convey the metaphoric sense R_1, G_0 and P_1, in opposition to FN which reciprocally indicates the sense R, G_0 and P_2. FN*aga* is also used reciprocally between speakers and hearers to convey the metaphoric sense R_1, G_0 and S_2 (negative solidarity), in opposition to FN which is also used reciprocally between speakers and hearers to indicate the sense R, G_0 and S_1 (positive solidarity). The referential-indexical meanings communicated in the use of these nonkin address terms are specified in these componential definitions by features on the R (sex of relative) and G (relative generation) dimensions, while the pure deferential indexical meanings are specified by features on the P (relative power) dimension and the pure solidarity indexical meanings are specified by features on the S (relative solidarity) dimension.

INDIRECT METAPHORICAL MEANING

To this point, the terms under analysis here have been referred to as nonkin address terms and the acts performed in the situated usage of these terms have been described as acts of addressing. This traditional anthropological view that kinship terms used by speakers in face-to-face interaction with hearers are "address terms" is in need of refinement. The characterization of this usage as accomplishing acts of addressing is too general. As Frake (1975:36–37) has pointed out, kin address terms are used to perform a variety of speech acts: they are used by speakers to greet hearers, to get their attention, to summon them, and so on.

The address usages of concern in this paper accomplish general acts of identifying or classifying—they are acts of classing hearers in semantic categories defined by conjunctions of distinctive features. They are "representative speech acts": they represent how something is, i.e., true or false, accurate or inaccurate; they are acts of fitting words to the world, of describing or representing the world (Searle 1976:10). Speech acts of this sort (e.g., acts of asserting, evaluating, reflecting) are most basically distinguished from speech acts of changing the world, of fitting the world to words (e.g., acts of requesting, committing, suggesting, legitimizing, and stipulating) (cf. Fraser 1974:144–149; Searle 1976:1–6).

It is useful in analyzing the usage of Yeniyurt nonkin address terms to distinguish three types of speech acts of describing the world (Fraser 1974:144–149). Assertive speech acts are acts indicating the speaker's strength of conviction in or committment to the truth of what he or she is describing or representing. Examples are acts of stating, asserting, insisting, affirming, claiming, denying and swearing. Evaluative speech acts are acts of indicating the speaker's assessment of the truth of what he or she is describing or representing and the basis upon which this judgment is made. Acts of declaring, appraising, estimating, formulating, analyzing, interpreting, assessing, judging, rating, and ranking are examples. Attitudinal speech acts are acts of indicating the speaker's attitude toward what he or she is describing or representing. Thanking, apologizing, complimenting, congratulating, blaming, cursing, praising, endorsing and denouncing are examples.

Speech acts may be direct or indirect (cf. Searle 1975). This distinction is also important here because nonkin address terms may be used to communicate the direct metaphorical meanings specified in the preceding section or to convey indirect metaphorical meanings. In direct usages, the speaker classes the hearer in the correct or appropriate semantic category. As shown above, for example, the use of FN*dayí* to a male hearer of the speaker's parent's generation who is the speaker's power superior classes the hearer as a member of the transferred metaphorical semantic category defined by the features R_1, G_{+1} and P_1. The use of FN*aga* to a male same-generation hearer who is the speaker's power superior classes the hearer as a member of the metaphorical semantic category defined by the features R_1, G_0 and P_1, while its use to a male same-generation hearer whose relationship to the speaker is nonsolidary classes the hearer as a member of the semantic category defined by the features R_1, G_0 and S_2. These direct usages accomplish evaluative speech acts: the speaker performs an act of classifying or identifying the semantic category membership of the hearer, fitting words to the social world and assessing the hearer's place in that world.

In indirect nonkin address usages, the speaker classes the hearer in an incorrect or inappropriate semantic category. The speaker performs an indirect speech act by way of performing an act of classifying. Since the speaker's usage classes the hearer in a false category, the speaker's primary point cannot be to perform a direct act of identifying the category membership of the hearer, but must instead be to perform an indirect speech act (cf. Searle 1975). This categorical falsity is the means by which

metaphorical meanings are shifted or switched. When the metaphorical sense normally conveyed by the use of nonkin address terms is ruled out by categorical falsity, certain indirect metaphorical senses can be conveyed by implication (for categorical falsity and implicational meaning, see Grice 1975).

When transferred metaphorical semantic categories are shifted in this way, a distinct variety of metaphorical meaning is communicated. This variety of metaphorical meaning, which is to be distinguished from transferred metaphorical meaning, may be termed "indirect metaphorical meaning." In the use of nonkin address terms in Yeniyurt, indirect metaphorical meanings are conveyed through categorically false acts of classifying which imply shifts of the features defining the transferred metaphorical senses of the terms. Feature shifts occur on only three of the dimensions of contrast which differentiate the transferred senses of the terms. These three types of metaphorical switches of transferred meanings are illustrated in the following examples. A categorical switch involving a reversal of the power features P_1 and P_2 occurs in the use of FN*aga* to a male, same-geneneration hearer, who is a power inferior of the speaker; this marked usage implies that the hearer's power relative to the speaker is greater than it is in actuality and, thereby, conveys the indirect metaphorical sense "increased power relative to the speaker." A switch of categories involving a reversal of the solidarity features S_1 and S_2 occurs in the use of FN*aga* to a male, same-generation hearer, whose relationship to the speaker is solidary; this marked usage implies that the relationship between the speaker and the hearer is more distant or less intimate than it is in actuality and, thereby, conveys the indirect metaphorical sense "decreased solidarity in relation to speaker." And a categorical switch involving a shift of the generation features G_0, G_{+1} and G_{+2} occurs in the use of FN*dayí* to a male, nonintimate or power superior hearer, who is in approximately the same generation as the speaker; this marked usage implies that the generational distance between the speaker and the hearer is greater than it is in actuality and, thereby, conveys the indirect metaphorical sense "increased age-status relative to speaker." These indirect usages accomplish attitudinal speech acts: in implying increases and decreases in relative power, relative solidarity and relative generation, the speaker indirectly performs acts of flattering, praising, complimenting, honoring, elevating, and the like, on the one hand, and acts of denigrating, insulting, degrading, defaming, deflating and so forth, on the other. In these usages, the speaker fits words to the social world, describing or representing the hearer's place in it and expressing his or her attitude toward it.

SUMMARY AND CONCLUSION

In summary, it has been shown that the social meaning conveyed in the situated usage of kinship terms may be accounted for in terms of several concepts borrowed from sociolinguistics and the theory of speech acts. It was argued that kin terms communicate meaning in speech acts and that underlying every sentence, including those containing a kin

term, is an abstract structure (hypersentence) which makes reference to the speaker and the hearer. It was maintained that the meaning conveyed in kin term usage is governed by the social relationship between the speaker and the hearer to whom reference is made in the hypersentence, even when this reference to participants is not specified explicitly in the speaker's utterance. Usage of kin terms in addressing nonkin in Yeniyurt was shown to communicate transferred metaphorical meanings derived from the genealogically-defined meanings conveyed by the terms in addressing kin, and to accomplish both the referential-indexical function of identifying nonkin hearers as the referents of the terms in situations of usage and the pure indexical function of indicating the relative power and/or relative solidarity relation between speakers and hearers. In addition to and by way of these direct evaluative speech acts of identifying the semantic category membership of hearers, the nonkin address terms were also shown to be used in performing indirect attitudinal speech acts of flattering, praising and honoring or denigrating, insulting and degrading. The figurative meaning communicated in these usages was termed indirect metaphorical meaning because it is conveyed through categorically false acts of classifying in which transferred metaphorical semantic categories are shifted. In conclusion, it may be suggested that this approach to the analysis of the transferred and indirect metaphorical meanings communicated in the use of kin terms in Yeniyurt provides at least a partial answer to the "usage objection" to formal semantic analysis.

NOTES

1. The field research upon which this paper is based was supported by National Institute of Mental Health Research Fellowship MH43154–02 and Research Grant MH11895–01. This paper is extensively revised from its original publication in 1975 and includes material presented at the Mathematical Social Science Board Conference, "Kinship Semantics," held in Riverside, California, December, 1972. I would like to thank the follwoing conference participants for their valuable comments: Cecil H. Brown, Herbert Clark, Roy G. D'Andrade, Hugh Gladwin, Paul Kay, and Frank Wordick. I would particularly like to thank Charles O. Frake and David B. Kronenfeld for detailed critiques. Most of all, I would like to express my gratitude to Bânû Özertuğ without whose help the data could not have been collected and without whose insights the analysis could not have been determined. While the paper has benefited from the suggestions of all these people, ultimate responsibility for the paper is, of course, mine alone.
2. While these questions may appear quite straightforward and obvious, their discovery was the result of an extended search. It should be emphasized that they are local village expressions, the significance of which may not be immediately obvious to Turks from such places as Istanbul and Ankara or to anthropologists who have worked in other areas of Turkey.
3. The kin terms are transcribed in standard Turkish orthography; the symbol *ı* is used here for the Turkish "dotless *i*."
4. Several additional address forms are also used within and without the village. For example, older alters are sometimes addressed as *bey* or *beyefendi* (Mr.) and *hanım* or *hanımefendi* (Mrs./Miss), especially city alters upon first acquain-

tance, and younger alters are occasionally addressed as *oğlum* ('my son') and *kízím* ('my daughter'). The omission of these terms in no way affects the point of this discussion.

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12

'Wise Words' of the Western Apache: Metaphor and Semantic Theory

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> The greatest thing by far is to be a master of metaphor. It is the one thing that cannot be learnt from others; and it is also a sign of genius, since a good metaphor implies an intuitive perception of the similarity in dissimilars.
>
> —Aristotle *(Rhetoric, 3)*

INTRODUCTION

The subject of metaphor, wrote Michel Bréal (1964:127), "is inexhaustible." And so it would seem, for ever since the fourth century B.C., when Aristotle expressed his opinion that metaphor was the highest form of verbal art, this subtle instrument of language has been the object of serious thought and, in certain quarters, a source of richly informative

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debate.[1] Yet, as Clifford Geertz (1964:58) points out, despite the accomplishments of this venerable tradition of inquiry—a tradition, it should be added, which now boasts an enormous body of literature and embraces disciplines as diverse as philosophy, rhetoric, and literary criticism—the study of metaphor remains marginal to the major concerns of most social scientists. This lack of interest is both ironic and regrettable because today, perhaps more than at any time in the past, anthropologists and linguists are in a position to construct theoretical models that can contribute in useful ways to a clearer understanding of the semantics of metaphor and the role of metaphor in cultural systems (see, for example, Geertz 1964, 1972; Lévi-Strauss 1963, 1966; Hale 1971; Scheffler and Lounsbury 1971; Scheffler 1972; Fernandez 1972, 1974).

I assume that an adequate theory of metaphor must articulate with a theory of language—and by direct implication a theory of linguistic competence—that satisfactorily explains the ability of human beings to produce and interpret figurative speech. Such a theory has not yet been formulated within the paradigm of transformational linguistics nor does its development there seem probable. This is because the goals of modern semantics have been defined in such a way that a transformational grammar need only make explicit the tacit knowledge which enables the speaker of a language to assign literal or propositional meanings to sentence types (Katz and Fodor 1963; Chomsky 1965; George Lakoff 1971; Katz 1972). If such a grammar is taken to constitute a finished theory of language, as well as a complete model of the linguistic competence of its speakers, it is possible to conclude that the knowledge necessary to assign figurative meanings to sentences is "nonlinguistic" and that the ability to implement this knowledge lies outside the sphere of competence (e.g. Leech 1970: 89–93; Katz 1972:441–52). This is what a number of transformationalists have claimed or implied, and in so doing they have exempted themselves—and their theory—from having to explain the fact that many sentences in a language can be, are, and in some instances *must* be interpreted in ways that are not predictable from an understanding of their literal meanings alone.[2]

Simultaneously, an image has been created of man as "ideal speaker-hearer" (Chomsky 1965:3) which from an anthropological perspective is not ideal at all. Indeed, it is quite disturbing. For the ideal speaker-hearer as depicted by transformational grammarians is a person ("machine" is clearly a more accurate term) whose linguistic competence does not allow him to make meaningful sense of metaphorical speech. This hypothetical individual, we are informed, knows his language perfectly (Chomsky 1965:3). Yet this same knowledge, complete, unflawed, and impeccable as it purportedly is, presents him with no alternative but to interpret a sentence such as "Cops are pigs" as a piece of poppycock arising from a confusion of policemen with a class of barnyard animals. It stands to reason that as long as this image of the ideal speaker-hearer persists unmodified, and as long as linguistic competence is restricted exclusively to the kinds of information contained in transformational grammars, attempts to construct an adequate theory of metaphor will be severely hampered.

One possible solution to this problem is to enlarge the concept of competence to include the full complement of skills and abilities that permit the members of a speech community to assign nonliteral meanings to spoken and written messages. But what are these skills and abilities? And how are they to be discovered? This much seems clear. The knowledge required to interpret metaphor in a culturally appropriate manner cannot be prescribed a priori. Rather, it must be inferred on the basis of detailed investigations of how actual instances of metaphor are constructed and construed. Initially, we must go about our business empirically, determining for particular societies what kinds of statements count as metaphors, what kinds of statements count as interpretations or "explanatory paraphrases" (Urban 1939:39) of metaphors, and, perhaps most important of all, what kinds of standards are invoked to define some interpretations as more appropriate than others. In short, we must follow Hymes's (1964, 1971) suggestion and do "ethnographies of speech," in this case, ethnographies of metaphorical speech.

In this essay I shall outline a model for the analysis of metaphor and apply it to a small body of metaphorical statements made by residents of Cibecue, a Western Apache speech community located on the Fort Apache Indian Reservation in east central Arizona. Concomitantly, I will attempt to make more explicit what Aristotle and others have implied, namely that the production and interpretation of metaphorical speech involves a genuinely *creative* skill. Proceeding on the assumption that the successful interpretation of any metaphor entails the formation of at least one unitary concept, I will present evidence which suggests that these concepts are not lexically coded in the Western Apache language. On the basis of this discovery, I shall go on to argue that the interpretation of metaphor is grounded in an ability to form *novel semantic categories.* Such an ability is not accounted for by the standard transformational model of language, and I will conclude by discussing the implications of this deficiency for the revision of key linguistic concepts and the construction of an adequate theory of metaphor.

METAPHOR AS SIMILE

The most salient characteristic of metaphor consists in a blatant violation of linguistic rules that results in the expression of a proposition that is either logically false or, in Rudolf Carnap's (1955:47) terminology, "conceptually absurd." Walker Percy (1958:81) has put the matter nicely: a metaphor "asserts of one thing that it is something else" and is therefore inevitably "wrong." At the same time, of course, a metaphor is also "right" because, semantic disobedience not withstanding, the proposition it expresses can be construed as containing a truth. Interpreted one way, Thomas Brown's metaphor, "Oh blackbird, what a boy you are," is utter nonsense since it is simply not the case that blackbirds are boys, or vice versa. On the other hand, this statement can be taken to mean that despite numerous differences there is some sense in which boys and blackbirds are alike—in their penchant for loud noise, for example, or in their

propensity for energetic play. Herein lies a dilemma: how is it that a metaphorical statement can be at once both true and false?

This question is most commonly answered by asserting that metaphor is simile in disguise, a view which rests upon the more basic claim that an analytic distinction can be drawn between semantic features which compose the *designative* or *literal* meanings of words and features which compose their *connotative* or *figurative* meanings. Designative features are relatively few in number and serve as a set to specify the necessary and sufficient conditions for membership in the class of objects referred to by the word in question (Scheffler and Lounsbury 1971:4; Scheffler 1972:311). Connotative features are much more numerous, are nondefining in the sense just described, and consist of any and all "associated commonplaces" (Black 1962:32) or "contingent facts" (Katz 1972:285) which the designative meaning(s) of the word call(s) to mind.

If designative meanings are relied upon to interpret a metaphor, the reasoning goes, the proposition it expresses will be understood as false or, at worst, contradictory. However, if the metaphor is construed as a covert simile, it will be understood as expressing a noncontradictory proposition whose truth value can be assessed—and, for those in a position to do so, established—on the basis of connotative meanings. The trick to interpreting a metaphor, then, is to reject it as a declarative proposition (e.g. Blackbirds are boys), interpret it as a comparative proposition (e.g. Blackbirds are *like* boys), and confirm the truth of the latter by adducing at least one valid similarity between the classes of objects being compared (e.g. Blackbirds are like boys *by virture of* a shared fondness for whooping it up).

This theory has two important implications. One of these is that the same metaphor may be interpreted in different ways—as many, in fact, as there are features of connotative meaning shared by the metaphor's main constituents (Shibles 1971b; Richards 1948; Black 1962; Read 1952). We have interpreted Thomas Brown's metaphor on the grounds that blackbirds and boys share the attribute of being raucous, but other similarities could easily be adduced—liveliness, playfulness, an inclination to hide things, and so forth. It would be arbitrary, then, to insist that a metaphor has one "best" or "proper" sense; to the contrary, as many writers have noted, it is the special virtue of metaphor that it is capable of gathering unto itself several senses any one (or two, or three, or more) of which can serve as a basis for interpretation. This is a significant point because it accounts for the empirical fact that interpretations of a metaphor may exhibit wide variation even within the same speech community.

The theory before us also implies that the interpretation of a metaphor is ultimately grounded in an ability to form a concept (or two, or three, or more) which serves to establish an equivalence between the metaphor's main constituents, and of which, therefore, the constituents become exemplars par excellence (Brown 1958; Richards 1938; Urban 1939; Scheffler and Lounsbury 1971). In other words, if *X* and *Y* are the constituents equated in a metaphor, interpretation requires the formation of a concept that subsumes both *X* and *Y* and thus defines the terms of their identity.

Our interpretation of Brown's metaphor presupposes and derives from a concept that may be glossed as 'animate objects that are raucous.' It is essential to assume both the possibility and validity of such a concept. Otherwise, there would be no way to account for our particular interpretation of the metaphor, or to explain the fact that it can be readily understood by others.

The concepts required to interpret a metaphor are not expressed by the metaphor itself. Rather, they may be said to underlie it and must be *discovered* through the adduction of shared features of connotative meaning. It is this act of discovery, coupled with the sometimes puzzling search that precedes it, that can make the interpretation of metaphor an original and personal experience. And it is this same act of discovery—this "finding" of a meaning that resolves the puzzle—that endows metaphor with the capacity to cause surprise, to structure the perceptions of individuals in unanticipated ways, and to make them "see" associations they have never seen before. Thus, as Herbert Read (1952:23) has written, "A metaphor is the synthesis of several units of observation into *one commanding image;* it is an expression of a complex idea, not by analysis, or by abstract statement, but by a sudden perception of an objective relation" (italics added).

This brings us back to where we began. The meaningful interpretation of metaphor rests upon an ability to discern some element of plausibility or truth in a statement that asserts an implausibility or falsehood. It is clear, I think, that if we can characterize this ability—or, more precisely, if we can determine how the concepts that underlie the interpretation of metaphor are formed—we will have learned something interesting about metaphor itself. We will also have learned something interesting about cultural symbols and the way they work to impose order and meaning on that elusive entity sometimes known as the "real world."

'WISE WORDS' AS SIMILES

The class of Western Apache metaphors that will concern us in this essay takes the form of simple definitional utterances which represent expressions of a single surface syntactic type: subject + predicate + verb. In every case, the subject is a term designating some category of animate natural phenomena (e.g., *hadaditl'a':* 'lightning'; *kaage čo:* 'raven'; *koyiłčoože:* 'carrion beetle'), the predicate is a term designating some human category (e.g., *'iitsaa:* 'widow'; *'iškįįn:* 'boy'; *'indaa':* 'whiteman'), and the verb is a copula (*'at'ee:* 'is'; 'are'). In every case, too, a semantic rule is violated: subject and predicate do not agree. To put it more precisely, the subject possesses designative features that are incompatible with designative features possessed by the predicate. As a result, a proposition is expressed which is contradictory and therefore always false. Eight of these metaphors are presented below:

(1) a. *hadaditl'a' 'iškįįn 'at'ee* ('Lightning is a boy')
b. *kaage čo 'iitsaa 'at'ee* ('Ravens are widows')
c. *koyiłčoože 'indaa' 'at'ee* ('Carrion beetle is a whiteman')

d. *gooše čaγąąše 'at'ee* ('Dogs are children')
e. *mba'tsose 'indee 'at'ee* ('Coyotes are Western Apaches')
f. *doole č'ikii 'at'ee* ('Butterflies are girls')
g. *doole 'izeege 'at'ee* ('Butterflies are sweethearts')
h. *toołkaiyee saan 'at'ee* ('Burros are old women')

Metaphors of this type are identified by Western Apache as prime examples of what they call 'wise words' *(goyąąγo yaiti')*, a distinctive speech genre associated with adult men and women who have gained a reputation for balanced thinking, critical acumen, and extensive cultural knowledge. These persons, who form collectively a kind of intellectual elite, are typically well along in years and because of their advanced age are not expected to participate in the full round of daily activities that occupy most younger members of Western Apache society. Consequently, they have plenty of time for visiting and talking, especially with one another, and it is in the context of these conversational settings, called *baiyan diidała'at'ee* ('old people talking together'), that 'wise words' are used most frequently.

When an Apache—or an ethnographer—encounters a metaphor he does not understand, he may request an interpretation of it. The appropriate form of such a request is *XY haago 'at'eego dałęłt'ee?* (*X* and *Y*, how are they the same?), where *X* stands for the subject of the metaphor and *Y* stands for the predicate. The appropriate response is an explanatory paraphrase that describes one or more ways in which the referents of subject and predicate are alike. I recorded sixty-four of these explanatory paraphrases—one for every metaphor listed in (1a–h) from each of eight Western Apache consultants—of which the following are representative:

(2) a. *Metaphor: hadaditl'a' 'iškįn 'at'ee* ('Lightning is a boy')
Interpretation: Yes, young boys are the same as lightning. They both dart around fast and you just can't tell what they are going to do. They both act unpredictably. They never stay still. Both are always darting around from place to place. They will shoot aimlessly, too. They will both shoot anywhere, not aiming away from people's camps, not caring what they hit. That is why they both cause damage.

(2) b. *Metaphor: kaage čo 'iitsaa 'at'ee* ('Ravens are widows')
Interpretation: Ravens are widows, these people say. They say that because ravens and widows are poor and don't have anyone to get meat for them. That is why sometimes these women will stand near your camp and wait like that until you give them food. It is the same way with ravens. They stand around near roads so they can eat what is killed there; they just wait like that until some car hits something and kills it. That's what ravens are doing when you see them standing near roads. They are waiting to get fresh meat.

(2) c. *Metaphor: koyiłčoože 'indaa' 'at'ee* ('Carrion beetles are whitemen')
Interpretation: Well, there is this way that carrion beetle reminds us of whitemen—they waste much food. Carrion beetle, when

he is young and before he starts to eat meat, just eats a little hole in a leaf and then moves on to eat a little hole in another. He leaves plenty of good food behind him. It is like this with some white people, too. Another way they are the same, these two, is that in the summer they only come out from where they live when it is cool. You only see carrion beetles early in the morning and again in the early evening. It is the same with some white people. In the summer they always want to stay some place where it is cool.

(2) d. *Metaphor: gooše čaγąąše 'at'ee* ('Dogs are children')
Interpretation: I think this way about what that means. Both of them, children and dogs, are always hungry. They like to eat all the time, and when they don't get food they come to a place where someone is cooking. There is this way, too. Both of them get into everything and don't leave anything alone. So you have to shoo them away. If you don't, they might break something or soil it so you can't use it anymore.

These explanatory paraphrases, like all of the others I recorded, focus pointedly upon the specification of attributes in terms of which the referents of the constituents in a metaphor can be considered the same. Consequently, it seems safe to conclude that Western Apache metaphors of the type presented here are intended to be construed *as if* they were similes, and that failure to construe them in this manner will render interpretation extremely difficult, if not impossible. Of course, this does not explain how the concepts underlying particular interpretations are formed, or why some interpretations are judged more appropriate than others. I turn now to a consideration of these problems.

METAPHORICAL CONCEPTS: DESIGNATIVE FEATURES

The concepts that underlie interpretations of Western Apache metaphors consist of one feature of connotative meaning and one or more features of designative meaning. The connotative feature is criterial, is openly described in explanatory paraphrases, and may be viewed as the "outcome" of a search and selection procedure. . . . The designative features are noncriterial and are not described in explanatory paraphrases; they are contingent upon—and therefore must be inferred from—the position in lexical hierarchies of the semantic categories labeled by the metaphor's constituents.

Consider the following example. In paraphrase (2c) an equivalence is drawn between carrion beetles *(koyiłčoože)* and whitemen *('indaa')* on the grounds that both 'waste food' *(-'iit'an donaodi yodawołsida)*. 'Waste food' is a feature of connotative meaning since it does not state a necessary condition for membership in either of the categories 'whiteman' or 'carrion beetle.' But 'waste food' is only one component of the metaphorical concept. The other components consist of two features of designative meaning that together define a superordinate category to which both 'whiteman' and 'carrion beetle' belong, namely,

ni'gostsangoljihi ('living things that dwell on or below the surface of the earth'). Thus the complete metaphorical concept, defined by the features 'living thing' + 'earth dweller' + 'waste food,' is *ni'gostsangoljihi 'iitan donaodi yodawołsida* or 'living earth dwellers that waste food.'

The designative features of a metaphorical concept are always those features which the metaphor's constituents share by virtue of membership in the same semantic domain. As shown by the hierarchy presented in Figure 1, the referents of 'carrion beetle' and 'whiteman' do not become members of the same domain until the node labeled by *ni'gostangoljihi* ('living earth dwellers') is reached. At this point, they become exemplars of a single category, thus overcoming and resolving the semantic conflict—or, in Geertz's (1964:59) phrase, the "semantic tension"—that distinguishes them at subordinate levels. It follows that a metaphorical concept will always be *more inclusive* than either of the categories labeled by the metaphor's constituents. The concept 'living earth dwellers that waste food' is more inclusive than either 'carrion beetle' or 'whiteman'; it must be because it subsumes them both.

The hierarchical level at which the categories labeled by a metaphor's

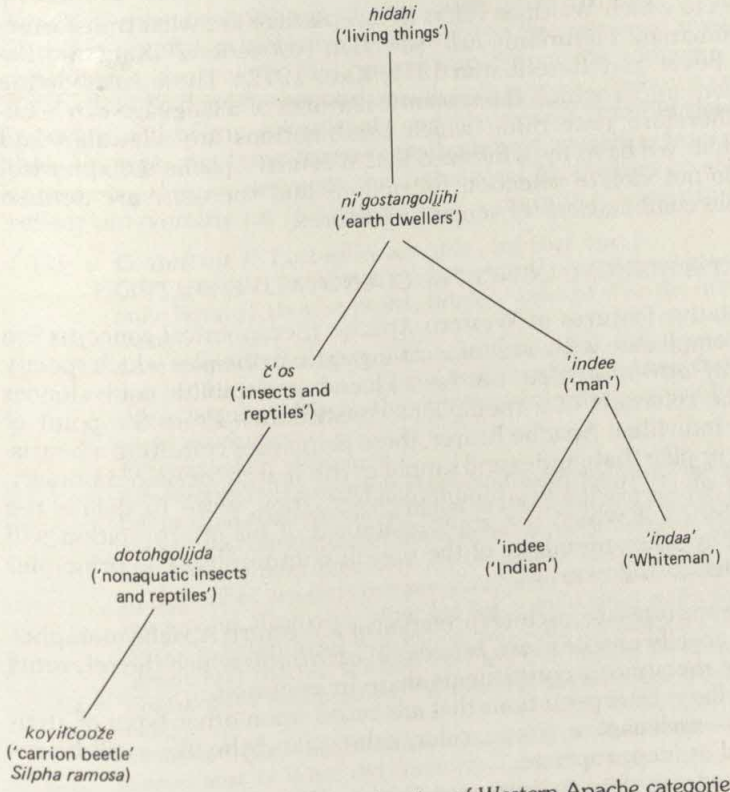

FIGURE 1. Lexical hierarchy showing location of Western Apache categories 'carrion beetle' *(koyiłčoože)* and whiteman *('indaa')*.

constituents are incorporated into the same domain is also the lowest level at which features of connotative meaning can be adduced. This is because shared connotative features must be compatible with designative features *shared by both constituents,* a condition that can only be satisfied when the referents of the constituents become exemplars of a single category. In terms of our example, this simply means that any connotative feature adduced to establish a similarity between carrion beetles and whitemen must be compatible with features that define 'carrion beetle' and 'whiteman' as 'living earth dwellers.' Any connotative feature that fails to meet this requirement (e.g., 'never die,' 'cause snowstorms,' 'make good cooking pots') will be prohibited.

This claim is supported by empirical evidence which will be presented later in the chapter. For the moment, however, let us agree with R. A. Waldron (1967:174) who has observed that:

> Metaphor is by no means carried out in total contravention of the rules [of a language] but in part by courtesy of the system itself. . . . Linguistic categorization involves classification on the basis of similarity of attributes. Metaphorical categorization is only an extension of normal linguistic activity; it is exceptionally *wide classification* [italics added].

The rules to which Waldron refers in this passage are what transformational grammarians commonly call "selection restrictions" (Katz and Fodor 1963; Bever and Rosenbaum 1971; Katz 1972). These rules define the conditions under which the semantic features of a language can cooccur, and therefore determine which combinations are allowable and which are not. We have hypothesized that Western Apache metaphorical concepts do not violate selection restrictions and therefore are defined by allowable combinations of semantic features.

METAPHORICAL CONCEPTS: CONNOTATIVE FEATURES

The connotative features of Western Apache metaphorical concepts are chosen in compliance with a set of sociolinguistic principles which specify the *kinds of attributes* that can be adduced to establish equivalences between the referents of a metaphor's constituents. From the point of view of the individual Apache hearer, these principles constitute a heuristic strategy or plan that guides and simplifies the search for shared similarities (Miller et al. 1960:159). Simultaneously, they serve to define the criteria in terms of which the appropriateness of his interpretation will be assessed by other members of the speech community. The principles may be stated as follows:

A. To be appropriate, an interpretation of a Western Apache metaphor must specify one or more *behavioral attributes* which the referents of the metaphor's constituents share in common.
B. (Corollary) Interpretations that are based upon other types of attributes—such as size, shape, color, habitat, and the like—will be rejected as inappropriate.
C. To be appropriate, an interpretation of a Western Apache metaphor must specify one or more behavioral attributes that are indicative

of *undesirable qualities* possessed by the referents of the metaphor's constituents.

D. (Corollary) Interpretations based upon attributes indicative of desirable qualities will be rejected as inappropriate.

The validity of principles A and C was given strong empirical support by regularities in the corpus of sixty-four explanatory paraphrases provided by my Western Apache consultants. Close examination of these statements revealed that principle A was never violated. In other words, all the paraphrases I recorded concentrated upon the identification of behavioral similarities and none adduced similarities of other kinds. Principle C was violated only twice, both times by the same consultant, and on each occasion his interpretation was challenged by older Apaches on the grounds that it "spoke too well" of the referents of the constituents of the metaphor in question.

Principles B and D were upheld by the Apaches' consistent rejection of metaphorical interpretations that violated the conditions defined by principles A and C. Discussions centering upon the reasons for such rejections were of special interest because, as shown by the three excerpts presented below, they yielded valuable information concerning the Apaches' own conceptions of how 'wise words' should be interpreted. The first excerpt, which describes an exchange between myself and two consultants, deals with the central importance of adducing behavioral similarities. So does the second, which presents a portion of a conversation between one of my consultants and his eleven-year-old daughter. The third excerpt, involving three consultants and myself, is from a discussion concerning the requirement that behavioral similarities be chosen which reflect unfavorably on the referents of a metaphor's constituents.

(3) a. *Consultant 1:* Butterflies are girls—try that one.
Ethnographer: Well, this is what I think of that one—they're both the same because they're pretty, brightly colored, like the dresses the girls wear.
Consultant 2: [Laughter] You could say that, but it sounds wrong to us . . . like when you said ravens are widows because they wear black. [More laughter] Maybe they are the same that way, but to us it doesn't mean anything.
Ethnographer: Why not?
Consultant 1: [Long pause] It doesn't mean anything because it doesn't tell us what they *do*, these things in the 'wise words.' You have to think about how they are the same in what they do—not what they look like. Think about how they act the same. That way you'll understand it. Butterflies are girls because sometimes they act mindlessly, just chasing around after each other having a good time when they should be working, helping out with chores and younger children. What they look like doesn't matter, it's how they *act* that makes them the same.

(3) b. *Consultant 1 (to daughter):* Who said it to you: carrion beetle is a whiteman?
Daughter: X's wife. She said it yesterday at her camp.
Consultant 1: What did you say?
Daughter: Nothing. I didn't know what she meant.
Consultant 1: Wise words. That old woman likes to talk that way. She

wanted to know if you knew how they were the same—whitemen and carrion beetles. Do you know how they are the same?
Daughter: No.
Consultant 1: Think.
Daughter: Because there are many of them.
Consultant 1: [Laughter] No. Think about what they do, those two—like leaving clear tracks so they are easy to follow, or like the way they waste food.
Daughter: But there *are* many of them.
Consultant 1: Yes. But when old people talk like that, using 'wise words,' they want you to think about how carrion beetle acts . . . then they want you to think about whitemen that same way. So you have to watch both of them, and then you will see how they are the same. It's what they *do* that matters. Now, do you understand?
Daughter: [Pause] I think so.

(3) c. *Ethnographer:* Yesterday I was talking about 'wise words' with Y, the medicine man's wife, and she said something I didn't understand. Maybe you could help me out.
Consultant 1: What did she say?
Ethnographer: She said these old people use 'wise words' when they want to say someting bad about someone.
Consultant 1: What made her say that?
Ethnographer: Well, she asked me why coyotes are Apaches and I said because they knew all about this country and were very smart.
Consultant 2: [Loud laughter] Maybe what you said about coyotes is true, but it doesn't sound right to us. Coyotes are like some Apaches who don't stay at home and roam around from place to place. Even at night they don't stay at home where they should be. They roam around from place to place and make lots of noise, yelling at night so you can't sleep. Some Apaches do like that, too, and it's no good; they should stay at home at night and keep quiet. That way they will stay out of trouble.
Consultant 3: It is true what that old woman told you. Those old people use 'wise words' when they don't like what someone has done. But they don't want to come out and use that person's name because that way he might hear about it and get angry. So they just say something like coyotes are Apaches and that is enough—everyone knows what they mean and who they are talking about.
Ethnographer: So when you are thinking about coyotes and Apaches you look for *bad* ways that make them the same? You look for what they do that is *no good?*
Consultant 2: Yes. I thought you knew that. It's the same for all 'wise words'. . . One time when we were talking you said butterflies were girls because they were both pretty. That was wrong because it's good to look nice. I don't think butterflies are pretty, but you do, and that made us laugh. It's because they chase around after each other, like they had no work to do, that makes them the same. And that's no good.
Consultant 3: Let me tell you a story. One time my mother was sick and went to the hospital in Whiteriver. It was before my older sister got married. She was supposed to look after us, cook for us. She did all right, but then one day she took off with my two [female] cousins and they went to where some people were getting ready for a dance. They stayed there all morning. Then they went to another camp to

> drink beer with some boys. Then they went to another camp. At night they went back to the dance. Finally, they came home. My grandmother had come to take care of us, and I guess she knew that my sister had been running around. When my sister came in my grandmother didn't say anything at first. Then she said to my older brother, "Butterflies are girls and one of them just flew in." My sister knew what it meant, I guess, because she started feeling bad. . . . That's how they use 'wise words,' these old people—when they want to say something bad about someone.

It should now be apparent that the principles which govern the selection of connotative meanings are closely related to what Western Apaches conceive to be the proper use of 'wise words' in ordinary conversation. As the material presented above suggests, metaphors of this type are regarded by Apaches as vehicles for the expression of mild personal criticism. It is criticism of a highly oblique sort, however, and the identity of the person (or persons) being denounced can only be inferred from other kinds of information. This is a complex and often extremely subtle process whose analysis falls beyond the scope of the present study. What is significant here is that the requirement to select behavioral attributes—and, more specifically, behavioral attributes indicative of undesirable qualities—arises directly from the purposes 'wise words' are designed to serve and the objectives they are intended to accomplish in the course of social interaction.

LEXICAL GAPS AND NOVEL SEMANTIC CATEGORIES

Philosophers of language have long insisted that both the existence and the use of metaphor can be understood as a response to incompleteness in the vocabularies of natural languages (e.g., Urban 1939; Ullman 1962; Alston 1964; Henle 1962). No vocabulary, these scholars point out, is ever without interstices or "holes," and metaphor—because it communicates meanings that the literal senses of available words do not—functions to close or "plug" them. In this way, metaphor serves to counter what Uriel Weinreich (1964:57) has called the "designative inadequacy" of lexical systems. Simultaneously, it alleviates this deficiency through the introduction of new meanings, thus extending the semantic range of language and increasing its expressive potential.

Despite the widespread acceptance of this proposition, no systematic attempts have been made to test it (Shibles 1971b). A search of the pertinent literature reveals that the validity of the proposition—or how one might go about demonstrating it—are matters which are simply not discussed. This does not seem to worry the philosophers, but for those of us who like to season our propositions with a dash or two of verification it inevitably raises doubts. In an effort to dispel such doubts, I shall present evidence here which indicates that Western Apache metaphors do, indeed, operate in the manner described above. Specifically, I will suggest that the concepts underlying these metaphors correspond to *accidental lexical gaps* in the Western Apache lexicon.

In a stimulating essay, Bever and Rosenbaum (1971) have discussed

the subject of lexical gaps in detail. A lexical gap is defined as any combination of semantic features that is not labeled by a lexeme. Some unlabeled combinations are prohibited by the selection restrictions of a language and in this sense may be considered *systematic* lexical gaps. Other unlabeled combinations are perfectly allowable (that is, they are not prohibited by selection restrictions) and therefore may be regarded as *accidental* lexical gaps. Our hypothesis, then, can be confirmed under two conditions. First, we must present evidence which demonstrates that Western Apache metaphorical concepts are composed of allowable combinations of semantic features. Second, we must show that these concepts are not labeled by lexemes.

To test the first condition, I went through my corpus of explanatory paraphrases and made an inventory of all the concepts used by my consultants to interpret the metaphors listed in (1a–h). Then, following the procedure outlined above . . . I defined these concepts in terms of their semantic components and constructed descriptions of the definitions in Western Apache (see appendix). Finally, I presented these descriptions to my eight Apache consultants, asking each of them if, according to his or her understanding, 'such things could be found in this world.' For example:

Ethnographer: Living earth dwellers that waste food—from what you know, can such things be found in this world?
Consultant: Yes.

An affirmative response such as this was taken to mean that the concept in question was linguistically and culturally valid—in other words, that it represented an allowable combination of semantic features and that it denoted a class of objects that existed, or could exist, within the Western Apache universe. A negative reply would have been taken to mean just the opposite. In fact, however, there were no negative replies. My consultants were unanimous in their agreement that all of the descriptive phrases listed in the appendix made clear and legitimate reference to 'things found in this world.' On the basis of this unblemished consensus, I conclude that Western Apache metaphorical concepts do not violate selection restrictions and are composed of allowable combinations of semantic features. This satisfies the first condition of our hypothesis.

To test the second condition, I began by asking my consultants if the phrases in the appendix could be said in shortened form. For example:

Ethnographer: Living earth dwellers that waste food. This, what I have just said, can it be said in a short way and still mean the same?
Consultant: No, I don't know of a short way to say it.

This query, which was designed to elicit synonyms or near synonyms that were also unitary lexemes, met with only two positive replies. One consultant explained that phrase 3.5 ('living earth dwellers that act fearlessly'; *ni'gostsangoljihi dobiłgoyeeda*) could be replaced by the term *dobiłgoyedahi* ('fearless things'). Another consultant said that *dogoyąądahi* ('mindless things') could be substituted for phrases 6.3 and 7.1 ('living earth dwellers that act mindlessly'; *ni'gostsangoljihi dogoyąąda*).

I also asked my consultants if the referents of the phrases in the appendix had 'names' (*'iži'*). For example:

Ethnographer: Earth dwellers that waste food—do these things have a name?
Consultant: No, they have no name, just what you have called them.

In Western Apache, a name, whether it denotes a person, animal, supernatural power, or whatever, typically consists of a unitary lexeme, and for this reason my query was understood as a request to provide linguistic forms that were more compact than the rather lengthy concept labels I had composed. The query was successful in uncovering five names, all of which were unitary lexemes. Two of these are the lexemes that replace phrases 3.5, 6.3, and 7.1 (see above). The other three are as follows:

(4) a. *tsitahohi* ('loners'; 'recluses') replaces phrase 2.6 ('living things that keep to themselves'; *hidahi tsitaaho*).
b. *biłgoyeihi* ('lazy things') replaces phrase 6.4 ('living earth dwellers that refuse to work'; *ni'gostsangoljihi do'ičeγodadiida*).
c. *haškeihi* ('mean things') replaces phrase 8.2 ('living earth dwellers that are short-tempered'; *ni'gostsangoljihi dabižǫǫ dabik'e*).

It should be noted that the lexemes which replace phrases 6.3 and 7.1, 6.4, and 8.2 are not strict synonyms because, as further questioning of my consultants revealed, these terms can be used to designate any and all 'living things' and are not restricted exclusively to 'earth dwellers.' If we choose to assign significance to this fact, we may conclude that 30 of the 33 metaphorical concepts in the Appendix are not labeled by unitary lexemes; if we choose to ignore it (as my consultants apparently did) the number of unlabeled concepts falls to 28 of 33. In either case, it is clear that a high percentage of the concepts in our sample are not lexically coded. This establishes the second condition of our hypothesis and, together with the evidence presented earlier in support of the first condition, serves to substantiate our claim that Western Apache metaphorical concepts correspond to accidental lexical gaps.

This finding has several important implications. One of these is that the interpretation of most Western Apache metaphors—and, I would venture to guess, the interpretation of most metaphors that take the form of disguised or hidden similes—requires of the individual an ability to form *novel semantic categories.* This must be so since the concepts that make interpretation possible are not lexically coded and therefore are not accessible through the meanings of existing vocabulary items. Almost certainly, it was this lack of accessibility that Aristotle had in mind when he said that metaphor—and here, of course, I am assuming he meant metaphorical concepts—could not be learned from others.

One can, of course, receive instruction in how to interpret metaphors from other people. This is what explanatory paraphrases and some English professors are all about. But because metaphorical concepts lack names, such coaching is inevitably circuitous. Consequently, even with instruction (and most assuredly without it) metaphorical concepts must be attained on one's own through private acts of discovery and recognition that reveal the existence of relationships where previously none were perceived. Such acts are *creative* in the fullest and most genuine sense, for they presuppose and exemplify an ability to arrange familiar semantic features into unfamiliar combinations, to form fresh categorizations of

not so fresh phenomena—in short, to generate new categories of meanings.

Once again Aristotle perceived the fundamental point, this time when he observed:

> Most smart sayings are derived from metaphor and thus from misleading the hearer beforehand. For it becomes evident to him that he has learned something when the conclusion turns out contrary to his expectations, and his mind seems to say, "How true it is! but up to now I missed it" (*Rhetoric 3;* quoted in Shibles 1971a: 123).

It is the rare gift of the maker of metaphor—the "mark of genius" that Aristotle so admired—that he glimpses new categories of meaning before anyone else and, realizing that the linguistic resources at his disposal are inadequate to express them directly, turns to metaphor as a way to escape from his dilemma. His task is not an easy one, however, for by voicing a proposition that is literally false he runs the risk of being completely misunderstood. The maker of metaphor speaks in semantic contradictions and extends to his audience an invitation to resolve them. If the invitation is accepted, and if attempts at resolution are successful, the result is the acquisition of a concept that is in a very real sense unspeakable. Herein lies the essential ambiguity of metaphor and also its ultimate force—it "says" with ordinary words what ordinary words alone cannot say, thus facilitating the conversion of apparent absurdity into understandable truth.

Clifford Geertz (1964:59) has spoken to this same point but in a slightly different way. Writes Geertz:

> The power of metaphor derives precisely from the interplay between the discordant meanings it symbolically coerces into a unitary conceptual framework and from the degree to which that coercion is successful in overcoming the psychic resistance such semantic tension inevitably generates in anyone in a position to perceive it. When it works, a metaphor transforms a false identification . . . into an apt analogy; when it misfires it is a mere extravagance.

TOWARD A THEORY OF METAPHOR

At the outset of this essay, I claimed that transformational linguistic theory could not explain the ability of human beings to produce and understand figurative speech. I also claimed that the transformational model excluded certain types of information which are essential to an adequate theory of metaphor. In conclusion, I would like to return to these issues and discuss them in relation to the foregoing analysis of Western Apache 'wise words.'

Let us suppose that we have in hand (or in head) a transformational grammar of the Western Apache language consisting of a syntactic component and a semantic component which conform in all respects to the specifications set down by Chomsky (1965) and Katz (1972). Let us also suppose that we are presented with the sentence *hadaditl'a' 'iškjɨn 'at'ee* ('Lightning is a boy') and are asked to interpret it. How would our gram-

mar respond? What would it tell us? It would respond by assigning the sentence a literal reading. Specifically, it would inform us that the sentence is contradictory because the subject possesses semantic features that are incompatible with features possessed by the predicate (e.g., 'sky dweller' vs. 'earth dweller'). This is *all* our grammar could accomplish. A figurative reading of the sentence would be out of the question. Why?

The inability of our grammar to provide figurative readings is directly related to a guiding principle of transformational linguistics which states that the goal of semantic theory should be to predict and explain the *properties and relations of sentence types.* "A semantic theory," according to Katz (1972:7), "must explain why the meaning of a linguistic construction makes it a case of a certain property or relation, makes it exhibit the phenomenon of synonymy, ambiguity, contradictoriness, and so forth." Such a theory, Katz (1972:62) goes on to say, ". . . must abstract away from features of utterance contexts and concern itself with inherent features of sentence types."

This conception of the aims of semantic theory is coupled with a principle by means of which transformationalists determine what constitutes "semantic" information and what does not. This principle is described by Katz (1972:285–86) as follows:

> Semantic theory offers . . . a principle telling when a piece of information is part of the meaning of a lexical item, as opposed to being a factual comment on its referent. Given such a principle, each piece of information that qualifies as semantic by the principle enters the semantic representation of that item in the form of a semantic marker in one of the lexical readings in its dictionary entry. . . . This principle, then, must decide how to choose between two lexical readings R_1 and R_2, which are the same except that R_2 contains *(ex hypothesi)* a component, putative semantic marker μ that represents *factual* information and R_1 does not contain μ. . . . Linguistics should resist the addition of any information to a lexical reading if its inclusion would fail to increase the predictive or explanatory power of the lexical reading and thus of the dictionary. Since what must be predicted and explained by a dictionary, as part of the semantic component of a grammar, are the semantic properties and relations of anomaly, synonymy, analyticity, entailment, etc., by showing that R_2 does not predict or explain any of these properties or relations that are not already predicted and explained by R_1, it will be shown that μ, which is the only difference between R_1 and R_2, represents factual, not semantic, information. . . . If we cannot eliminate information from the lexical reading of a word W without losing predictions and explanations of properties and relations of expressions in which W occurs, then such information belongs in the dictionary entry for W and is hence rightly deemed semantic; and if we can simplify the entries of the dictionary by excluding a certain piece of information from all of them, then it is rightly deemed non-semantic.

In actual practice, the principle Katz discusses is used by transformational grammarians to exclude from dictionary entries all "factual comments" about the referents of lexical items. Such information is considered "non-semantic" because it is not needed to explain the properties and relations of sentence types. For example, with respect to *hadaditl'a'* *'iškiin 'at'ee* ('Lightning is a boy'), the fact that both boys and lightning 'dart around' *(-naadamase)* is not necessary to demonstrate that the sen-

tence as a type exhibits the property of contradictoriness. In this way, transformationalists have defined as extraneous to semantic theory what we have been calling connotative meanings or, as Katz (1972:451) puts it, ". . . the contingent properties that groups of people think belong to the referents of words." In so doing, they have committed themselves to a rigidly literalist view of language in which figurative speech has no place and cannot be explained. A theory of metaphor is *ipso facto* impossible. Is it any wonder that our grammar of Western Apache can tell us only that 'Lightning is a boy' embodies a semantic contradiction? Or, as Uriel Weinreich (1966:399) has observed, that transformationalists have been constrained to deal with ". . . special cases of speech—humorless, prosaic, banal prose"?

It would be a serious mistake, however, to conclude that transformational theory is irrelevant to a theory of metaphor. On the contrary, it can play a significant role. For reasons that should now be clear, a theory of metaphor must be capable of assigning to the constituents of a metaphorical expression their literal or designative meanings. To the extent that a transformational grammar specifies these meanings, it forms a necessary part of a theory of metaphor. Also, since metaphor involves the violation of certain selection restrictions, a theory of metaphor must be capable of identifying the restrictions that are violated. Transformational grammar can accomplish this task with considerable precision (at least in principle) and for this reason, too, is needed in a theory of metaphor. It is apprent, however, that a theory of metaphor must do more than specify the literal meanings of words and the rules determining their cooccurrence; it must also account for the adduction of connotative meanings and specify the rules that govern their combination with designative meanings to form metaphorical concepts. It is this last set of requirements that a transformational grammar does not satisfy.

But let us suppose it did. For the sake of argument, let us imagine that our hypothetical Western Apache grammar contains a complete listing of designative and connotative features for every item in the lexicon, as well as a full set of selection restrictions that define the conditions under which these features can be combined. Could we now assign a figurative interpretation to 'Lightning is a boy'? We might, but we would have no assurance whatsoever that our interpretation would be culturally appropriate. For nowhere in our grammar is it stated that the connotative meanings adduced to interpret a Western Apache metaphor must be commensurate with behavioral attributes that reflect unfavorably upon the referents of the metaphor's constituents. This vital information is omitted entirely, and its absence points to another weakness in the transformational paradigm: a total neglect of the cultural norms and attitudes that influence the ways in which language is *used*.

This neglect is an unfortunate consequence of the transformationalist dictum that a theory of linguistic competence should concern itself exclusively with sentence types and ignore both sentence tokens and the contexts in which they occur. Such a proscription serves to disengage the study of language from social life, and neatly removes from consideration and analysis all forms of knowledge that guide and shape the activity

of speaking, the actual conduct of verbal communication. Gone at a blow are the skills that enable the members of a speech community to decide on what should be said and how to say it, to whom it should be said and under what circumstances, and how what is said should be interpreted. Gone, in other words, are the kinds of sociolinguistic principles that complement grammatical knowledge but go beyond it to define what is proper, fitting, and, as we have seen in the case of the Western Apache, fundamental to the evaluation of metaphorical speech. Gone is what Teeter (1970) has called the *command* of language, the ability to call upon it and apply it in ways that meet the standards and expectations of fellow speakers. Wrenched from their natural context like so many fish out of water and hung up for sale, the sentence types prized by transformationalists are arrayed and analyzed as if they had no place of origin, no relationship to the affairs of men, no purpose but to be dissected and, once laid open, awarded to the cleverest bidder. They exist in an ecological vacuum, isolated and unconnected, their formal properties and relations still intact, but their appropriateness—if ever they had any—a quality of no importance, a matter of no concern.

The drawbacks inherent in such an approach emerge most clearly when we consider metaphor in relation to the concept of linguistic competence. Chomsky (1965) restricts competence to the tacit knowledge represented in a grammar, that is, to the body of rules and information which enables a speaker-hearer to generate and interpret an infinite number of novel, well-formed sentences. It is in this sense, of course, that he has characterized language as a productive and creative system. Note, however, that if we accept Chomsky's definition we are obliged to conclude that because Western Apache metaphors are designatively ill formed they constitute evidence of flawed or imperfect competence and therefore reflect *deficiencies* or *defects* in the Apaches' "knowledge" of their language. In this view, metaphors are reduced to the level of linguistic *errors* or *mistakes* not unlike those a child might make. And far from being regarded as manifestations of a creative skill, they can be taken as indicative of just the opposite: an inability to speak according to grammatical rules. Here is a real dilemma. Either we declare all Apaches who use metaphor to be linguistic incompetents, or we admit that the concept of competence as defined by Chomsky (1965) is unsatisfactory and in need of modification. The latter is clearly the only alternative.

What is needed, as Hymes (1967, 1971, 1972, 1973) has suggested, is an expansion of the concept of linguistic competence to include the ability to speak *appropriately* as well as grammatically. The two should not be confused because appropriate speech is often ungrammatical and grammatical speech is frequently inappropriate. Part of an Apache's sociolinguistic competence is to know when it is appropriate to violate grammatical rules and which rules are appropriate to violate. It is this kind of knowledge that he acts upon when speaking metaphorically. 'Wise words' are prime examples of *appropriately ill-formed* utterances.

In the broadened view of competence proposed by Hymes, metaphor would no longer count as a linguistic blunder; neither would it imply impaired linguistic knowledge. To the contrary, it would be seen as a

positive achievement, a linguistic invention won through the artful exploitation of connotative meaning and a willingness to challenge grammatical authority. Such exploitation and challenge is not for linguistic cripples; it is for persons whose competence allows them to experiment with linguistic principles in unorthodox ways. Since this ability presupposes intimate familiarity with the principles themselves, metaphor implies mastery of language rather than ineptness.

If the study of metaphor can contribute to a widened interpretation of linguistic competence, it can also contribute to a deeper appreciation of linguistic creativity. Chomsky (1965) has equated creativity with the ability of human beings to produce and understand an infinite set of novel utterances. Metaphor, on the other hand, entails the invention and acquisition of novel semantic categories. The two abilities, though related, are not the same. The former, as Hymes (1973:119) has pointed out, is basically concerned with the "systemic potentiality" of language, while the latter is ". . . that kind of creativity which consists of the discovery of possibilities implicit in a [linguistic] system, but not yet discovered, not yet known" (Hymes 1973:99). Creativity in Chomsky's sense consists in the unfolding of existing structures. Creativity in metaphor consists in the use of existing structures to forge new ones.

What distinguishes the two even more sharply, I think, is that the former is achieved through adherence to grammatical rules while the latter is achieved by breaking them. Chomsky's type of creativity manifests itself in the form of utterances which, novel though they may be, do not depart from established canons of designative meaning. In contrast, metaphor flaunts these canons in order to capitalize on connotative meanings and expresses what designated meanings cannot. Herein lies a fundamental difference. The transformationalist's ideal speaker-hearer is consistently obedient to the strictly grammatical rules of language. The maker of metaphor is not.

As defined by Chomsky, creativity is commensurate with one form of "unboundedness" in language, that is, with the ability of individuals to make continuously original use of materials already present. Metaphor entails this ability, too, but *adds* to what is present by formulating and sponsoring new categories of meaning. In so doing, it functions to augment the lexical semantic resources of linguistic systems, and serves as an indispensable device for adapting these systems to the changing communicative needs of their speakers. Chomsky's ideal speaker-hearer is content with his language as he finds it and does not disturb the *status quo*. The maker of metaphor is dissatisfied with his language and seeks to enrich it through the introduction of untried means of expression.

It hardly needs to be said that cultural anthropologists owe a great intellectual debt to Noam Chomsky and his insight into parts of a theory of language. But metaphor cannot be ignored, and Chomsky's theory, though necessary to an explanation of it, is not sufficient. What the Western Apache tells us is true. Ravens *are* widows and lightning *is* a boy. I am a carrion beetle. And it is important to find out why. Our best hope for success lies in continuing ethnography, and this means trying to make sense of contingent facts, unlabeled semantic categories, rules for lan-

guage use, and the like. For it is in metaphor—perhaps more dramatically than in any other form of symbolic expression—that language and culture come together and display their fundamental inseparability. A theory of one that excludes the other will inevitably do damage to both. And this, we can all agree, should not be allowed to continue.

In closing, it is well to keep in mind that well-turned metaphors are recognized and appreciated by persons other than ethnographers, philosophers of language, and literary critics. Shortly before I last departed from the field, one of my Western Apache consultants, who was standing by as I packed my belongings, suggested that I stop for a moment—there was someting he wanted to tell me:

> It's too bad that you didn't try to learn about wise words before. When I was young, old people around here used to make them up all the time. Only a few of them did it and they were the best talkers of all. Some people would try but they couldn't do it so they stopped trying. Now you know what some 'wise words' mean—that's what you asked us to teach you. But you still haven't made any up. Maybe those old people would have taught you how they did it. It's hard to do and you have to know a lot about everything. Those old people were smart. One of them would make up a new one and right away other people would start to use it. They had never heard it before and if it was a good one it would make them happy and they would laugh. It's still that way when someone makes one up. Only the good talkers can make them up like that. They are the ones who *really* speak Apache. They are the ones who make up 'wise words' and don't have to use someone else's. I don't know how they do it. It's something special that they know.

APPENDIX:

Definitions and Descriptions of Western Apache Metaphorical Concepts

1. Metaphor: *hadaditl'a' 'iškįįn 'at'ee* ('Lightning is a boy')
   - 1.1 'living thing' + 'dart around'; *hidahi naadamase* ('living things that dart around')
   - 1.2 'living thing' + 'shoot aimlessly'; *hidahi dailįįyo dadiłtoh* ('living things that shoot aimlessly')
   - 1.3 'living thing' + 'cause damage'; *hidahi dawąąne dayiłčǫǫ* ('living things that cause damage')
   - 1.4 'living thing' + 'act unpredictably'; *hidahi haga'aadadešįį dobegoziida* ('living things that act unpredictably')
2. Metaphor: *kaaye 'iitsaa 'at'ee* ('Ravens are widows')
   - 2.1 'living thing' + 'unable to procure fresh meat'; *hidahi doyo'itsį' nadadineeda* ('living things that are unable to procure fresh meat')
   - 2.2 'living thing' + 'wait to be given food'; *hidahi 'iit'anbigąą č'idadodil'įį* ('living things that wait to be given food')
   - 2.3 'living thing' + 'talk through nose'; *hidahi bičįįyeyadałti'* ('living things that talk through their nose')
   - 2.4 'living thing' + 'talk in raspy voice'; *hidahi dihedlaat'goyadałti'* ('living things with raspy voices')
   - 2.5 'living thing' + 'tire easily'; *hidahi dos'aa daiyaada* ('living things that tire easily')
   - 2.6 'living thing' + 'keep to themselves'; *hidahi tsitaaho* ('living things that keep to themselves')

3. Metaphor: *koyiłčoože 'indaa' 'at'ee* ('Carrion beetles are whitemen')
   3.1 'living thing + 'earth dweller' + 'waste food'; *ni'gostsangolįįhi 'iitan donaodi'yodaawołsida* ('living earth dwellers that waste food')
   3.2 'living thing' + 'earth dweller' + 'avoid heat'; *ni'gostsangolįįhi gostoodbit'inčǫǫ* ('living earth dwellers that avoid heat')
   3.3 'living thing' + 'earth dweller' + 'leave discernible tracks'; *ni'gostsangolįįhi bike'dabigozįį* ('living earth dwellers that leave discernible tracks')
   3.4 'living thing' + 'earth dweller' + 'make winding paths'; *ni'gostsangolįįhi dailįiyo'anataał* ('living earth dwellers that make winding paths')
   3.5 'living thing' + 'earth dweller' + 'act fearlessly'; *ni'gostsangolįįhi dobiłgoyeeda* ('living earth dwellers that act fearlessly')
4. Metaphor: *gooše čaγąąše 'at'ee* ('Dogs are children')
   4.1 'living thing' + 'earth dweller' + 'are always hungry'; *ni'gostsangolįįhi j'ida'dadnlįį* ('living earth dwellers that are always hungry')
   4.2 'living thing' + 'earth dweller' + 'get into everything'; *ni'gostsangolįįhi dawąą neyanakai* ('living earth dwellers that get into everything')
   4.3 'living thing' + 'earth dweller' + 'do whatever they want' (i.e., act in an undisciplined manner); *ni'gostsangolįįhi dobadesdiida* ('living earth dwellers that do whatever they want')
5. Metaphor: *mba'tsose 'indee 'at'ee* ('Coyotes are Western Apache men')
   5.1 'living thing' + 'earth dweller' + 'run around at night'; *ni'gostsangolįįhi tl'ege' anakaał* ('living earth dwellers that run around at night')
   5.2 'living thing' + 'earth dweller' + 'make noise at night'; *ni'gostsangolįįhi tl'ege' hadagoł'aa* ('living earth dwellers that make noise at night')
   5.3 'living thing' + 'earth dweller' + 'stay away from home'; *ni'gostsangolįįhi gowąąyo donastąąda* ('living earth dwellers that stay away from home')
   5.4 'living thing' + 'earth dweller' + 'act sneaky and sly'; *ni'gostsangolįįhi mba'tsose ga'adat'e* (living earth dwellers that act sneaky and sly')
   5.5 'living thing' + 'earth dweller' + 'play tricks'; *ni'gostsangolįįhi łąągote'iłco* ('living earth dwellers that play tricks')
   5.6 'living thing' + 'earth dweller' + 'get into trouble'; *ni'gostsangolįįhi nagontl'og yanat'liž* ('living earth dwellers that get into trouble')
6. Metaphor: *doole jekii'at'ee* ('Butterflies are girls')
   6.1 'living thing' + 'earth dweller' + 'go around in pairs'; *ni'gostsangolįįhi nakii gonadakai* ('living earth dwellers that go around in pairs')
   6.2 'living thing' + 'earth dweller' + 'always want to play'; *ni'gostsangolįįhi nadawolzežǫǫ hadat'ii* ('living earth dwellers that always want to play')
   6.3 'living thing' + 'earth dweller' + 'act mindlessly'; *ni'gostsangolįįhi dogoyąąda* ('living earth dwellers that act mindlessly')
   6.4 'living thing' + 'earth dweller' + 'refuse to work'; *ni'gostsangolįįhi do'ič' egodadiida* ('living earth dwellers that refuse to work')
7. Metaphor: *doole 'izeege 'at'ee* ('Butterflies are sweethearts')
   7.1 'living thing' + 'earth dweller' + 'act mindlessly'; *ni'gostsangolįįhi dogoyąąda* ('living earth dwellers that act mindlessly')
   7.2 'living thing' + 'earth dweller' + 'chase after each other'; *ni'gostsangolįįhi łiłnadade* ('living earth dwellers that chase after each other')
   7.3 'living thing' + 'earth dweller' + 'can't concentrate'; *ni'gostsangolįįhi donatsi'dakesda* ('living earth dweller that can't concentrate')
8. Metaphor: *toołkaiye saan 'at'ee* ('Burros are old women')
   8.1 'living thing' + 'earth dweller' + 'rest with head hanging down'; *ni'gostsangolįįhi binii' nadaz'ąąna hanaadayoł* ('living earth dwellers that rest with their heads hanging down')
   8.2 'living thing' + 'earth dweller' + 'are short-tempered'; *ni'gostsangolįįhi dobidawołtǫǫda* ('living earth dwellers that are short-tempered')

8.3 'living thing' + 'earth dweller' + 'are independently minded'; *ni'gostsan-golịịhi dabižǫǫdabik'e* ('living earth dwellers that are independently minded')

NOTES

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2. This position is most obviously characteristic of "interpretive semantic theory" as outlined by Chomsky (1965) and developed by Katz (1972). It should be noted, however, that many of the criticisms expressed in this essay apply with equal force to "generative semantic theory" (e.g., George Lakoff 1972; Reddy 1969) as well. Recently, generative semanticists have broadened their approach to the analysis of meaning to include features of discourse but, as Joel Sherzer (personal communication) has aptly observed ". . . they have not yet really faced up to the basic issue of the *use* of language in social and cultural life, and until they do the models they propose will be inadequate to deal with such crucial uses of language as metaphor."

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IV
Variability and Change

In the introduction to Section I, it was stated that culture exists in the minds of individual society members, but that the cultural models of these individuals are not identical. In much anthropological research, just as in most linguistic research, the descriptions of investigators are idealized composites abstracted from this variability found in the cognitive models of individuals. Most often, anthropological and linguistic descriptions attempt to account for the perfect knowledge of an ideal individual in a culturally homogeneous community. This is a heuristic assumption justified by the fact that it permits investigators to procede with detailed analyses.

COMPOSITE AND SHARED MODELS OF CULTURAL KNOWLEDGE

The description of culture as ideal composite can be illustrated with data from research on the ethnobotanical classification system of the Ndumba, a New Guinea Highlands society (Hays 1976, 1979). Working with ten informants, five male and five female adults matched for age and marital status, Hays compiled a lexicon of 1,247 names for classes of plants. Each of these terms is considered by at least one informant to be a label for an Ndumba ethnobotanical category, but the entire set of terms is not known by any one of the ten informants. The complete set of 1,247 Ndumba plant names collected from the ten informants is thus a composite lexicon known in its entirety only by the ethnographer. An ethnographic description of Ndumba ethnobotany that reports 1,247 plant names is a *composite* model of the ideal cultural knowledge of an omniscient informant in what is treated as a culturally homogeneous community (Hays 1976:489).

Ethnographers sometimes adopt an alternative strategy in describing a culture. Rather than construct a composite model that is an abstraction away from the details of individual variability, they construct a model that includes only cultural elements that are shared by all society members and excludes all those that are not uniform for all members. Cultural descriptions of this type can also be illustrated by the research on Ndumba plant classification. If only the ethnobotanical terms that are recognized by all ten informants are listed, the Ndumba plant lexicon consists of only 970 terms or 77.8% of the terms in the composite

lexicon (Hays 1976, p. 491). An ethnographic description of Ndumba ethnobotany that reports 970 plant names is a *shared* lexicon; that is, it is a model of a subset of cultural knowledge that is shared by members of a presumably homogeneous community (Hays 1976, p. 489).

The majority of anthropological studies of cognitive systems, including those considered so far in this book, are composite or shared models of cultural knowledge. Both composite and shared models ignore individual variability and analyze cultural knowledge as though it were homogeneous in the community. Adopting this simplifying assumption has made significant progress possible in research and has considerably advanced our understanding of linguistic and cultural phenomena. But viewed from the perspective of individual society members, composite models overrepresent, and shared models underrepresent, the cultural knowledge that is controlled by any single member of society. No individual Ndumba, for example, knows all of the 1,247 terms in the composite lexicon, and every Ndumba knows more than the 970 terms in the shared lexicon (Hays 1976). This is not to say that either model is false or invalid; both have, in fact, contributed important insights into cognitive structures and processes. The data on individual variability omitted from composite and shared models are, however, crucial to an understanding both of how the diverse cognitive models of individuals are organized in societies (cf. Wallace 1970) and of how this individual variability serves as a basis for linguistic and cultural change (cf. Labov 1972, also see Kay, Chapter 14 following).

In the studies in this section, it is generally assumed that cultural knowledge is heterogeneous. The focus of attention is on variability in the cognitive systems of individual community members and on the role of this variability in linguistic and cultural change. Before turning to these topics, however, it is first important that we confront what Michael D. Rose and A. Kimball Romney term the issue of "individual differences" versus "cognitive pluralism." This is the problem of distinguishing between situations in which there is true variability in cognitive structures among society members and situations in which there are multiple cognitive models that are shared by society members.

INDIVIDUAL DIFFERENCES VERSUS COGNITIVE PLURALISM

Rose and Romney, in Chapter 13, are interested in demonstrating for one domain that what might appear to be an example of individual variability in cognitive structures is in fact an example of shared cognitive structures that are utilized as contextual alternatives. The domain they investigate is American English kinship terminology. As indicated in the General Introduction and in the introduction to Section II, two models of the cognitive structure encoded in the American English kinship terminology—one described by Anthony F. C. Wallace and John Atkins (1960) and the other by A. Kimball Romney and Roy G. D'Andrade (1964)—have been extensively discussed and debated in the anthropological literature since the early 1960s. The principal issue in this controversy is "psychological validity." Given these alternative models, both determined in careful and thorough componential analyses, the problem is to decide which of them is psychologically real. When, as in the case of these componential analyses, it is determined through independent psychological and behavioral research that both models represent cognitive structures in the minds of society members, the prob-

lem then is to decide whether the models represent individual differences or cognitive pluralism. This latter is the problem for which Rose and Romney provide a solution in their chapter.

Two Models of American English Kin Terms

The two models are quite distinct in several ways. For the eight basic terms for male consanguineal relatives (*grandfather, grandson, father, son, uncle, nephew, brother,* and *cousin*) the Wallace-Atkins model posits only two dimensions of contrast: *generation,* which has the values b_1 (two generations superior), b_2 (one generation superior), b_3 (zero generation), b_4 (one generation inferior), and b_5 (two generations inferior), and *descent,* which has the values C_1 (lineal), C_2 (colineal), and C_3 (ablineal). As defined by Wallace and Atkins (1960, p. 348–349), lineals are persons who are direct ancestors or descendents of Ego, colineals are nonlineals whose ancestors include, or are included in, all the ancestors of Ego, and ablineals are consanguineal relatives who are neither lineals nor ablineals. The Romney-D'Andrade model posits three dimensions of contrast: *descent,* which has the values direct (lineal) and collateral (all nonlineal consanguineals), *generation,* which has the values G_{+2} (two generations removed), G_{+1} (one generation removed), and G_0 (same generation), and *reciprocity,* which has the values +reciprocal (senior member of reciprocal pair) and −reciprocal (junior member of reciprocal pair). For the Rose and Romney study, the crucial difference between these models is the presence in one and the absence in the other of an "Ego reference point." Ego occupies an explicit position in the Wallace-Atkins model; it is defined as b_3 (zero generation) and C_1 (lineal), and it is the specific reference point in relation to which the terms are defined. In the Romney-D'Andrade model, an Ego reference point is absent or only implicit, and the terms are defined in relation to each other.

As stated, the aim of the Rose and Romney study is to determine whether these two models occur in the population of American English speakers as variant models of the kinship domain that differ from individual to individual or as multiple models of the domain that are shared by all individuals. They hypothesize that, if the latter, (the cognitive pluralism interpretation) is correct, the responses of subjects on similarity judgment tests in which Ego reference is present will be consistent with the structure of the Wallace-Atkins model, and those of subjects on tests in which Ego reference is absent will be consistent with the structure of the Romney-D'Andrade model. To test their hypothesis, Rose and Romney administered similarity judgment tests to groups of undergraduate college students in which the kin terms were presented with and without Ego reference. Judgments of similarities among terms without an Ego reference point were elicited by asking, e.g., "Which two of the following terms are most similar or which one is most different: *uncle, cousin, nephew?*" Judgments of similarities with an Ego reference point were elicited by asking the same types of questions about explicitly referenced kin terms, e.g., "Which two of the following terms are most similar: *Pat's uncle, Pat's cousin, Pat's nephew?*"

Data from similarity judgment tests may be subjected to *multidimensional scaling analysis,* a complex statistical technique that produces a visual representation of the similarity relationships among the terms. Rose and Romney's application of MDS to their similarity judgments data (particularly to their rating-scale dyads data) provides strong support for the cognitive pluralism hypothesis. The

MDS model of similarity judgments with an Ego reference point closely resembles the Wallace-Atkins two-dimensional model of American English kinship semantics; the MDS model of similarity judgments without an Ego reference point is very much like the Romney-D'Andrade three-dimensional model. This is a clear indication that both models are psychologically real for the population as a whole and that their use is determined by the presence or absence of Ego reference. So Rose and Romney's conclusion is that when what they term "cognitive context"—presence vs. absence of ego reference—is taken into consideration, there is no variability among individual cognitive models of American English kinship; there are simply multiple cognitive models that are shared by all American English speakers and used by them in distinct contexts.

SYNCHRONIC VARIABILITY AND DIACHRONIC CHANGE

The next two chapters in this section both investigate true synchronic variability and its relation to diachronic change. The relation between linguistic variation and language change is described with notable clarity and precision by Paul Kay in Chapter 14. Kay's purpose in this chapter is to relate an updated account of the evolution of basic color terms to the findings of recent research on the role of variation in language change. As he points out, research on this topic has been concerned almost exclusively with phonological and syntactic variation (see, for example, Labov 1969 and 1972, G. Sankoff 1974, Cedegren and Sankoff 1974, and D. Sankoff 1978), and little attention has been devoted to lexical variation. Kay clearly demonstrates that the general finding in this area of research—that the source of all linguistic change is synchronic heterogeneity in the speech community—is valid for the color lexicon as well.

As pointed out in the introduction to Section II and in Chapter 6, Brent Berlin and Paul Kay's (1969) seminal research on basic color term universals led them to conclude that there are eleven universal focal color categories and that there is a universal sequence of seven stages in which these categories are encoded in language history: black and white are the first focal colors encoded (Stage I), red is always the third (Stage II), green and yellow in either order are the fourth and fifth (Stages III and IV), blue is the sixth (Stage V), brown is the seventh (Stage VI), and purple, pink, orange, and grey are encoded in any order to bring the total to eleven (Stage VII). Before considering synchronic variation in connection with the growth of color lexicons, Kay revises this universal encoding sequence to take into account recently acquired color-term data. An important change in this new scheme is that the sequence is now comprised not only of focal categories but of composite categories as well (composite categories, which are categories that include more than one of the universal focal colors, are distinguished from focal categories by the use of capital letters; focal categories emerge from these composite categories to form additional categories). In the revised sequence, WHITE (which includes white and most light and warm colors) and BLACK (which includes black and most dark and cool colors) are the first categories encoded (Stage I), RED (which emerges from WHITE and includes red and warm colors) is the third (Stage II), the fourth and fifth are yellow and GRUE, a composite category which includes green and blue (Stages III and IV), the sixth is green or blue, depending on

which emerges from the GRUE category (Stage V), the seventh is brown (Stage VI), and the addition of purple, pink, orange, and grey in any order brings the total to eleven (Stage VII). (Further refinements of the universal encoding sequence are discussed in Witkowski and Brown 1977 and Kay and McDaniel 1978.)

The research findings on phonological and syntactic variation lead Kay to make several predictions about variability and change in basic color terms. Distinguishing between generally more homogeneous speech communities, which are diachronically stable and exhibit only minimal individual variability, and generally more heterogeneous speech communities, which are in the process of change and exhibit significant individual variability, Kay makes several predictions. He predicts that in homogeneous speech communities speakers will share the same set of basic color terms and the most salient secondary terms will be the next ones in the evolutionary sequence. For example, if society members share a Stage I BLACK and WHITE system, the most salient secondary term will be the Stage II term RED, followed by Stage III terms yellow and GRUE, and so on. Kay also predicts that in heterogeneous speech communities speakers will not share the same set of basic color terms but will have different sets of terms that are contiguous in the evolutionary sequence. For example, if some society members have a four-term Stage III system, others may have five-term Stage IV systems and six-term Stage V systems, but not ten- or eleven-term Stage VII systems. In heterogeneous speech communities of this sort, Kay predicts that there will be a statistical correlation between size of color lexicon and age, the color lexicons of younger society members being more advanced than those of older members. As will be seen in his chapter, analyses of all the available color-term data confirm Kay's predictions and strongly support the view that synchronic variability in the speech community is the source of linguistic change.

Kay's analysis focuses on variation data that would be omitted from both shared and composite models. He does not limit his account to basic color terms that are shared by all society members, nor does he construct a composite model that lumps together all the color terms known by at least one society member; instead, he analyses the distribution of actual synchronic variation in the community to determine the source and direction of diachronic change.

Ethnic Identities in a Heterogeneous Community

The approach described above is also taken by Roger Sanjek in his analysis of the ethnic domain in urban Ghana. Sanjek's study, in Chapter 15, is unique in that it examines variability and change in an extremely heterogeneous community. Unlike most anthropological field research, which is done in small-scale, face-to-face communities, the research reported by Sanjek was done in a city of 650,000 people. Sanjek collected data on "tribes" (which he calls "ethnic identities") from 423 residents of the Adabraka section of Accra, who identify themselves with 23 different ethnic groups and speak 14 different languages. In response to the question, "Can you tell me the tribes living in Accra today?" 57 informants provided highly variant lists of tribes. Of the total of 136 tribe terms supplied by these informants, only six were mentioned by at least half of the informants and only twenty-three by one-tenth of them. The tribe classification systems of these informants also varied widely: one-third of the informants

organized the domain as a taxonomic hierarchy (there was, however, great diversity in these taxonomic systems); the rest used a variety of other modes of organization.

From the disparate informant lists of tribes, Sanjek is able to construct a composite model of the ethnic domain, which he terms a "larger cultural information system." This model represents the sum of the knowledge of tribes of all the Adabraka informants; it is not a model of the knowledge of any single informant (cf. Sanjek's Figures 2 and 3, pp. 310 and 311). He also presents a shared model, which, he argues, is an implicit structure underlying the empirical variability in the lists and modes of organization. This shared model is evident, Sanjek argues, in the fact that informants are uniform in their recognition of tribes from all three major language groups in southern Ghana.

In addition to presenting shared and composite models of the Adabraka ethnic domain, Sanjek also considers the extreme diversity of individual models of the domain in relation to diachronic change. He takes the position that tribe terms that occur very infrequently in the lists, and individual models that differ greatly from most of the other models, may be regarded as "remnants of the past" or "harbingers of the future." The model of a 70-year-old informant that is organized in terms of the seven traditional sections of the old town around which Accra has grown is a "fossil" remnant of a typical classification system of the early 1900s, not an irrelevant idiosyncrasy. And the models of a few educated informants that are based on ethnic categories used in the wider society (for instance, in government censuses or in the schools) are also of considerable interest because they will probably be common models of the ethnic domain in the future. In analyzing synchronic variability in ethnic classification systems in this way, Sanjek is able to shed light on both the course that diachronic change has taken from the past to the present and the direction that it is likely to take in the future.

THE GROWTH OF ETHNOBOTANICAL CLASSIFICATION SYSTEMS

The growth and development of ethnobotanical categories in language history are analyzed by Cecil H. Brown in the final chapter in this section. The aim of his article, Chapter 16, is to contribute to a recent and developing line of research that has attempted to establish that categories in particular semantic domains are encoded in languages in certain universal sequences. This direction of research was initiated by Brent Berlin's (1972) seminal article, "Speculations on the Growth of Ethnobotanical Nomenclature." Basing his speculations on evidence from a number of languages, Berlin argues convincingly that in the histories of individual languages ethnobotanical ranks (which were described in the introduction to Section II and in Chapter 4) are encoded in a fixed order: generic rank taxa are always the first and, in the early stages of language history, may be the only categories encoded in folk botanical classification systems; life-form and specific taxa are encoded after generic taxa; intermediate and varietal taxa are encoded after life-form and specific taxa; and the unique beginner, or kingdom, taxon is encoded last and generally late in language history. Berlin (1972, p. 53) diagrams this sequence as follows:

$$\text{generic} \rightarrow \begin{Bmatrix}\text{life-form}\\ \text{specific}\end{Bmatrix} \rightarrow \begin{Bmatrix}\text{intermediate}\\ \text{varietal}\end{Bmatrix} \rightarrow \text{unique beginner}$$

All ranks (except, of course, the kingdom rank) are open classes to which categories are added throughout the history of a language; for example, generic taxa continue to be added after life-form and specific taxa, intermediate and varietal taxa, and the kingdom taxon have been encoded. There is no temporal ordering of life-form versus specific taxa or intermediate versus varietal taxa in this encoding sequence, but at least one life-form and one specific must be encoded before any intermediates or varietals are added, and at least one intermediate and one varietal must be encoded before the addition of the kingdom taxon.

The principal concern in Brown's chapter is with the historical growth of life-form categories in the Mayan language family. He assumes the universal encoding sequence of ethnobotanical ranks outlined by Berlin and focuses on the universal order in which categories within the life-form rank are encoded. Brown's analysis of this encoding sequence, which is specified in detail in an earlier article (Brown 1977), is familiar from the research we have already considered on the universality and evolution of basic color terms. (Recall Leech's discussion of Berlin and Kay's original work in Chapter 6 and Kay's reformulation of this work in Chapter 14). There is a substantive universal set of five ethnobotanical life-form categories that are encoded in languages in a fixed order. The five universal botanical life-form taxa are tree (large, ligneous, or woody plants), GRERB (small, herbaceous or leafy plants), bush (intermediate size plants), grass (herbaceous plants with bladelike or spear-shaped leaves), and vine (creeping, climbing, or twining plants); the sequence in which these categories are encoded is the following: at first, there are no life-forms (Stage I), tree is the first life-form category encoded (Stage II), GRERB, a composite category like GRUE, which includes grasses, herbs, and most other nontree plants, is encoded next (Stage III), and bush, vine, and grass are then added in no set order (Stages IV, V, and VI). The "tree" and "GRERB" categories in early-stage languages are generally broader than they are in later stage languages, because when the "bush," "vine," and "grass" categories are added they include plants classed earlier as "tree" and/or "GRERB."

In his article, Brown reconstructs the botanical life-form terminologies for the protostages in the history of the Mayan language family (*proto-* is used to indicate reconstructed forms of languages), and demonstrates that historical change in Mayan life-form categories occurred exactly as predicted by the universal encoding sequence. Tree was the only life-form term in proto-Mayan, the parent Mayan language spoken some 4,000 years ago, and it continued to be the only life-form in the daughter languages for 3,000 years. Life-form categories began to be added to the lexicons of Mayan languages about 1,000 years ago, and in every case the first life-form added was GRERB, followed by one or several of the other life-forms, bush, vine, and grass. All thirty-one contemporary Mayan languages now have at least two life-form categories and some have three, four, and five.

What is particularly interesting about Brown's findings is that most of the terms for life-form categories have been added to the Mayan ethnobotanical lexicon within the last 1,000 years or so. They are, thus, a relatively recent development in Mayan language history, which can be assumed to date from

about 10,000 to 20,000 years ago (Witkowski and Brown 1978, p. 445). There is evidence that this is also true for the development of life-form terms in other language families; Brown (1979) has recently presented evidence that life-forms also are added to ethnozoological classification systems quite late in language history.

To explain why life-form categories are late additions to ethnobiological lexicons, Brown employs a notion of "societal complexity," and demonstrates that simpler societies with low levels of political integration, social stratification and technological development have few life-form terms in their botanical lexicons, whereas societies that are politically, socially and technologically advanced have many botanical life-form terms (cf. Brown 1977, 1979). As Brown points out, societal complexity is a relatively recent world phenomenon, occurring during the last 4,000 years. With increased societal complexity there is an increased independence of humans from their biological environments and a correlated decrease in the number of plant species that society members can identify and name. Whereas in simpler societies individuals can identify and name as many as 500 to 1,000 separate plant species, in complex societies individuals can identify and name only 50 to 100 plant species (Witkowski and Brown 1978, pp. 445–446).

Berlin, as we have seen, describes the evolution of ethnobotanical classification systems as beginning with categories at the generic rank and from there adding categories upward and downward—that is, at more general and more specific ranks. Brown's analysis of the growth of life-forms depends additionally on Berlin's discussion of the "devolution" of ethnobotanical classification systems, in which he speculates that it is a process of decay that begins at the bottom of classification systems and moves upward—that is, from more specific to less specific ranks (Berlin 1972, p. 83). As societal complexity increases and dependence on the biological environment decreases, terms for specific and generic categories drop out of ethnobotanical lexicons, whereas terms for suprageneric categories (that is, life-form and intermediate taxa) become more numerous. As shown by Dougherty in Chapter 8, although generic rank taxa are most salient in simple societies, where there is frequent interaction with and direct dependence for subsistence upon the biological environment, suprageneric taxa are most salient in complex societies, where there is little interaction with or dependence upon the environment. So Brown's finding that life-form terms developed late in the history of the Mayan language family is explained as being the result of the increase in societal complexity that has occurred only within the last 4,000 years in human history.

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Cognitive Pluralism or Individual Differences: A Comparison of Alternative Models of American English Kin Terms

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This paper presents evidence that American English speakers have access to alternative cognitive structures of their own kinship system. The cognitive structure that is relevant at a given time is determined by context.

In an earlier paper on American English kinship terms, Romney and D'Andrade (1964:154) observed, "if individuals in a culture have alternative structures, it is possible that either different individuals have arrived at different cognitive structures, or that the same individual operates with alternative structures." We refer to the first concept as that of individual differences and to the second as that of cognitive pluralism. Both concepts may be valid, but our research supports that of cognitive pluralism.

Several empirical studies, utilizing multidimensional scaling techniques, have dealt with various factors related to the question of whether there exists one or more than one cognitive structure for a given semantc domain. Among these have been papers examining shifts in the semantic context of lexical items (Kirk and Burton 1977), age-related developmental changes (Howard and Howard 1977), variations in the method of eliciting similarity judgments (Henley 1969; Fillenbaum and Rapoport 1971; Miller 1977; Hutchinson and Lockhead 1977), and cross-cultural comparisons of semantic structure (Romney and Magana 1976). The research reported in these papers strongly suggests that under certain conditions, such as those created by contextual shifts and developmental changes, differences obtain in how individuals conceive of the items in a domain but that these can be accounted for by hypothesizing a single underlying structure.

As stated by Henley in a study of the domain of animal terms:

> the finding that similar structures are elicited by differing methods of investigation is important to research in semantics. There has often been a tacit assumption that there is a single structure for a given semantic field, irrespective of method, but there has been little empirical evidence supporting it. The results of the present experiment suggest that there is enough in common among different methods to support the view that a *common semantic structure exists*, irrespective of method, at least for such verbal items as were used here (Henley 1969:181–182, our emphasis).

Howard and Howard reported similar findings in terms of a single set of cognitive dimensions for the same domain of animal terms.

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> The present study focused on the development of the semantic structure underlying a set of animal names in as much as this domain has been investigated extensively in adults. The results reveal that the structure of this set of lexical items changes with development such that the perceptual feature of size becomes relatively less salient, whereas the more abstract features of predativity and domesticity become increasingly important (Howard and Howard 1977:112).

Evidence in favor of both a single structure and alternative cognitive structures has been given by Wexler and Romney (1972) for the domain of American English kinship terms. Wexler and Romney compared the similarity judgments elicited from subjects on eight male kinship terms with two structural (that is, theoretical) models of the terms that had been taken from Wallace and Atkins (1960) and from Romney and D'Andrade (1964). When they aggregated their data across the subjects, they concluded that a single model, namely, that of Romney and D'Andrade, best described the underlying structure of the kinship terms; when they analyzed the responses of individual subjects, they conjectured that subjects might be using either model. Wexler and Romney sum up their findings thus: "The most promising hypothesis seems, at least for kin terms, to be that there are a small number of structures (in our case, two) in the population, possibly one of them dominating, the second coming into play for some subjects at some times" (Wexler and Romney 1972:92). They do not, however, specify what affects the choice of kinship models by subjects.

Besides the Wexler and Romney study, there does not appear to be any multidimensional scaling experiment on the structure of semantic domains that has been interpreted as showing evidence for the existence of alternative cognitive structures, probably because of the assumption by researchers that each semantic domain is adequately conceived of as having a single structure (for example, Henley 1969:182). Even other multidimensional scaling experiments on American English kinship terms have assumed only a single cognitive structure, resembling the Romney-D'Andrade model (for example, Fillenbaum and Rapoport 1971; Nerlove and Burton 1972; Romney and Magana 1976).

But it is our finding, in agreement with Wexler and Romney, that alternative cognitive structures operate for American English speakers; we have further been able to establish the conditions necessary to create an alternation between different structures. The methods we used consist primarily of multidimensional scaling analyses of similarity judgments on American English kinship terms. The alternative cognitive structures are generated by varying the context for the kinship terms with different verbal references. Following Tyler (1969:91), we designate these contextual shifts by the term "cognitive contexts," because of the apparent effect they have on how subjects perceive the relationships among the kinship terms. The cognitive contexts are derived from the two kinship structural models, as described in the next section, and the responses of subjects on similarity-judgment tests are compared with those predicted by the two structural models.

The relevance of our research to the concepts (or models) of individual

TABLE 1. Wallace-Atkins Structural Model of American English Kinship (from Wallace and Atkins 1960).

	C1	C2	C3
b_1	Grandfather		
b_2	Father	Uncle	
b_3	(Ego)	Brother	Cousin
b_4	Son	Nephew	
b_5	Grandson		

differences and cognitive pluralism will be sufficiently clear. If it can be experimentally shown that the use of both kinship models by subjects would depend upon their individual or personal characteristics, then the hypothesis of individual differences will be corroborated. If it can be experimentally shown that the use of both kinship models by subjects would depend instead upon differences external to subjects and arising as variables in the general culture in which, presumably, they were originally socialized and in which, presumably, they continue to participate, then the hypothesis of cognitive pluralism will be corroborated.

THE KINSHIP STRUCTURAL MODELS

The American English kinship models we used are shown in Tables 1 and 2. They were previously derived by methods of componential analysis (Wallace and Atkins 1960; Romney and D'Andrade 1964) and differ both in the number of dimensions specified and in the values assigned to the meaning of the terms in each dimension. In Table 1, the Wallace-Atkins structural model identifies the two dimensions of generation (represented by the lower case "b"s) and of descent (represented by the capital "C"s). In Table 2, the Romney-D'Andrade structural model identifies the three dimensions of generation (represented by the capital "G"s), of descent, and of reciprocity. Because of the dimensional values from either model, no two include the same set of kinship terms, as is seen in Tables 3 and 4, the two models can be easily distinguished.

But the crucial difference between the two structural models for our purposes is found in the *presence or absence* of an "Ego reference point."

TABLE 2. Romney-D'Andrade Structural Model of American English Kinship (from Romney and D'Andrade 1964).

	Direct		Collateral	
	−Reciprocal	+Reciprocal	+Reciprocal	−Reciprocal
G ± 2	Grandson	Grandfather		
G ± 1	Son	Father	Uncle	Nephew
G 0	Brother		Cousin	

TABLE 3. Dimensional Values for the Wallace-Atkins Structural Model.

Dimension	Values	Kinship Terms
Descent	C1	Grandfather, Father, (Ego), Son, Grandson
	C2	Uncle, Brother, Nephew
	C3	Cousin
Generation	b_1	Grandfather
	b_2	Father, Uncle
	b_3	(Ego), Brother, Cousin
	b_4	Son, Nephew
	b_5	Grandson

It is this feature of the models that furnishes the cognitive contexts for the kinship terms. As Pelto says:

> From the diagrams above we notice that Romney and D'Andrade did not organize their analysis around that familiar reference point, "Ego." Without Ego, their system has no "zero point." The implication of this would seem to be that Ego is outside the system, looking at the interrelationships among the kin terms as a spectator; this perspective is quite different from that in which Ego is "in the middle," "looking around" at his kinsmen [as in the Wallace and Atkins model] (Pelto 1966:198).

Our major hypothesis may now be stated: when kinship terms are bound to an explicit Ego reference point, as in Table 1, the experimental results should be consistent with the theoretical structure generated by the Wallace-Atkins model; when an Ego reference point is absent or merely implicit, as in Table 2, the experimental results should be consistent with the theoretical structure generated by the Romney-D'Andrade model. By experimentally varying these cognitive contexts for the kinship terms, it should be possible to predict which model will in fact be used by the subjects.

In previously published studies of multidimensional scaling analyses

TABLE 4. Dimensional Values for the Romney-D'Andrade Structural Model.

Dimension	Values	Kinship Terms
Descent	Direct	Grandfather, Father, Son, Brother, Grandson
	Collateral	Uncle, Nephew, Cousin
Generation	$G \pm 2$	Grandfather, Grandson
	$G \pm 1$	Father, Son, Uncle, Nephew
	G 0	Brother, Cousin
Reciprocity	+Reciprocal	Grandfather, Father, Uncle
	−Reciprocal	Grandson, Son, Nephew

of American English kinship, subjects are reported to have been tested only on terms without an explicit Ego reference point. For example, a typical triad used for similarity judgments with unreferenced kinship terms would be "grandfather, son, grandson." As predicted above, these experiments have replicated the Romney-D'Andrade structural model (see Romney and D'Andrade 1964; Fillenbaum and Rapoport 1971; Nerlove and Burton 1972; Wexler and Romney 1972; and Romney and Magana 1976).

If subjects are to make similarity judgments in terms of the Wallace-Atkins Ego-referenced structural model, they must be presented with explicitly referenced kinship terms. Susan Haviland (n.d.) has suggested that explicit referencing of this kind could be obtained by prefacing each kinship term with the word "Ego's" or "someone's." Instead of an unreferenced kinship term such as "grandfather," then, we can test for "Ego's grandfather" or "someone's grandfather."

Because many subjects reported difficulties in processing kinship terms referenced by the word "Ego's" or "someone's," it was decided to substitute the proper name "Pat's," which has the advantage of avoiding sex bias while still preserving the notion of explicit referencing. The unreferenced triad given in the example above can be changed to the referenced triad "Pat's grandfather, Pat's son, Pat's grandson." Subjects are instructed to think of each of the kinship terms in the triad as referring to a relative of "Pat."

In the experimental research reported upon here, we used both triads and rating-scale dyads tests to collect data on similarity judgments. We have found nothing in the literature that raises any serious question about the validity of either method and, in fact, there are experiments that have demonstrated that the results yielded by the two methods largely coincide. Using animal terms, Henley (1969:183) concluded that the method of category ratings "and the method of triadic combinations are interchangeable from the standpoint of results." Hutchinson and Lockhead (1977:667) tested the two methods on bird terms and fruit terms, and they also reported that "dyadic and triadic measures give virtually the same results."

From our research on the referenced context for American English kinship terms, however, there is evidence that the method of triads does not always produce the same results as the method of rating-scale dyads. By comparing the structure of the two sets of referenced similarity judgments with the predicted structure for referenced kinship terms, namely, the Wallace-Atkins structural model, it was possible to determine that the rating-scale dyads test gives a more accurate measure of the relationships among referenced kinship terms than the triads test.

These findings in no way invalidate the studies cited above, because their comparisons of triads and rating-scale dyads dealt with unreferenced terms; our analysis of that context for kinship terms does not demonstrate any significant variations in results. But in research on referenced contexts (for example, Kirk and Burton 1977), it might be necessary to control for possible differences in the methods.

THE EXPERIMENT

Similarity judgments were elicited from ninety University of California, Irvine, undergraduate students in 1975–1976. Thirty students (fifteen males, fifteen females) participated in each of three experiments, one using referenced triads tests, one using referenced rating-scale dyads tests, and one using unreferenced rating-scale dyads tests. Testing was done at the Irvine Campus Social Science Laboratory, and students received class credit for volunteering their time. Data from 155 Irvine undergraduate students reported by Wexler and Romney (1972) were used for the unreferenced triads.

The kinship terms used in the experiments consisted of a set of the eight basic terms for male consanguineal relatives: "grandfather," "father," "son," "brother," "grandson," "uncle," "nephew," and "cousin." The referenced context was obtained by modifying the kin terms with the proper name "Pat's."

In both the referenced and unreferenced triads experiments, subjects were instructed to select out of each triad the term they judged least similar to the other two in meaning and were then tested on all fifty-six possible triadic combinations of the eight kinship terms. The pair of terms judged most similar in each triad was given a similarity score of one. In each set of fifty-six triads, there were twenty-eight unique pairs of terms that could receive a maximum similarity score of six; that is, each possible pair of the eight kinship terms was compared with the six other terms. Similarity scores for the twenty-eight pairs were aggregated across subjects in each triads experiment and then arranged in two (8 x 8) similarity matrices for input into the multidimensional scaling programs.

The referenced and unreferenced rating-scale dyads experiments were composed of the twenty-eight possible dyadic combinations of the eight kinship terms. Similarity scores were taken directly from subjects' ratings of the dyads on a scale from one to ten, on which the rating of greatest difference was one. The aggregated rating-scale similarity scores were arranged in two additional (8 x 8) similarity matrices.

The four kinship similarity matrices were analyzed by means of both two- and three-way multidimensional scaling techniques. It was possible to include for direct comparisons with the four matrices separate input matrices for the Wallace-Atkins and the Romney-D'Andrade structural models. These latter matrices were composed of theoretical similarity scores based on *process models* developed by Wexler and Romney (1972:75) for the purpose of "predict[ing] from any given structural model, the subject's performance on a cognitive task involving judgments of similarity or dissimilarity." The process models were derived by assuming that "two terms . . . relatively close together on a structural model . . . will be classed together [as more similar] more often." In effect, these models furnish a set of theoretical similarity scores that can be entered into the multidimensional scaling programs for comparisons with the other similarity matrices.

The two-way (n x n) multidimensional scaling analysis (Shepard 1962a, 1962b; Kruskal 1964) uses a rank order of similarities/dissimilarities on a set of stimulus pairs (here, the cells of a single matrix) to find a spatial configuration of the stimuli in a specified number of dimensions. Distances among stimuli in the spatial configuration reflect, to a greater or smaller degree, a monotonic transformation of the original dissimilarities so that in the multidimensional space the more similar stimulus pairs are in closer proximity.

The TORSCA two-way scaling program (Young and Torgerson 1967) employed in this study calculates a "stress" measure that indicates the extent to which the interpoint distances in the configuration represent a monotonic transformation of the original rank order of similarities. The minimum value of stress is zero, with increases in value indicating a less than perfect scaling.

Three-way (n x n x N) multidimensional scaling procedures (Carroll and Wish 1974; Wish and Carroll 1974) provide a more direct statistical measure of structural relationships among matrices constructed on the same set of similarity pairs. In this type of scaling, the similarity matrices are simultaneously compared in a common multidimensional space. According to Wish and Carroll (1974:450), "each matrix typically displays one subject's (or subgroup's) proximities [similarities] data, but it could be based instead on data from another kind of 'data source,' such as one of several experimental conditions, occasions, proximity measures, etc."

The INDSCAL three-way scaling program (Carroll and Chang 1970) we used computes a set of weights that numerically represent the saliences of each specified dimension for each of the input matrices. If the weights are always conceived of as positive coordinates, their expression for all pairs of specified dimensions in the form of two-dimensional plots in the positive quadrant is referred to as the "subject space." A zero weight indicates a dimension is not used, whereas an increase in weight corresponds to an increase in the salience of a particular dimension for a matrix. The meaning of the dimensions in terms of the "n" items can sometimes be interpreted in a "group stimulus space," which is also given in the form of two-dimensional plots for all pairs of specified dimensions.

Another statistic in the INDSCAL output that can be used to help determine the accuracy of a particular n-dimensional solution is a goodness-of-fit measure, VAF, that indicates the variance accounted for by the scaling solution.

RESULTS

In reporting the results of the two- and three-way multidimensional scaling analyses, the following symbols have been used to identify the four experimental test conditions and the two kinship structural models: "RSref" (referenced rating scales using "Pat's"). "RSunref" (unreferenced rating scales), "Tunref" (unreferenced triads), "Tref" (referenced triads), "*WA" (Wallace-Atkins model), and "*RD" (Romney-D'Andrade model).

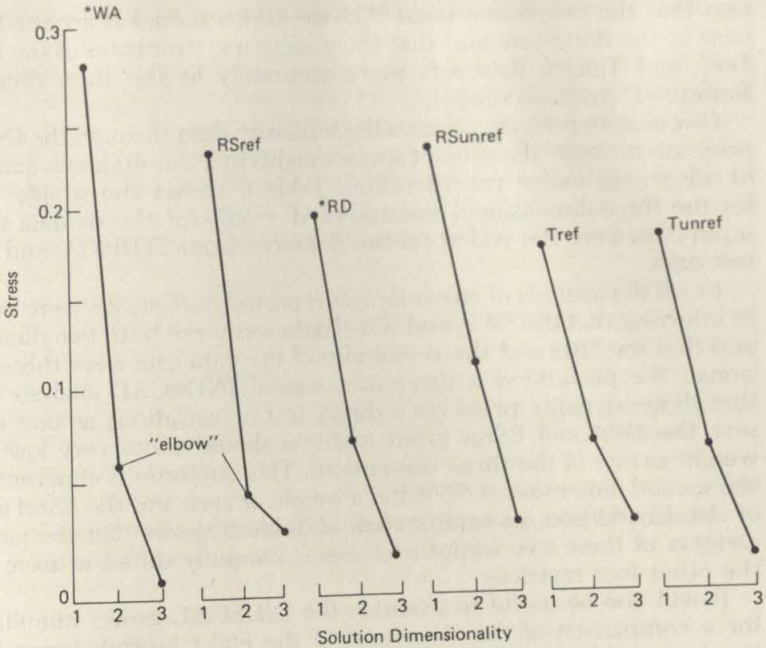

FIGURE 1. The stress values for two theoretical models and four samples plotted for three dimensions.

The Wallace-Atkins models were included by using the process models described previously.

Three-dimensional solutions from two-way scalings of the experimental data and models were compared by using stress values. We adopted Kruskal's (1964) proposal that a value of dimensionality should be selected for the input matrix that reduces stress to a reasonable figure and above which it is not significantly reduced. Such a dimensionality can usually be identified by plotting stress values against dimensionality and looking for an "elbow," or abrupt change in the curve.

The stress curves are shown in Figure 1, and the stress values given in Table 5. It can be seen that the *WA and the RSref curves "elbow" in dimension 2. (The elbow in *WA is partly obscured by the low stress value in the third dimension.) The other four curves indicate three-dimensional solutions, because their stress values do not become sufficiently small until the third dimension is extracted. This is one important indica-

TABLE 5. Stress Values in Three Dimensions.

		*WA	RSref	*RD	RSunref	Tref	Tunref
Dimensions	1	.004	.03	.01	.03	.03	.01
	2	.06	.05	.09	.12	.08	.08
	3	.28	.23	.21	.23	.18	.18

tion that the two-dimensional Wallace-Atkins model is accessed by subjects in the RSref task and that the underlying structures of the RSunref, Tref, and Tunref, data sets more accurately fit the three-dimensional Romney-D'Andrade model.

Our next step was to process the similarity data through the INDSCAL program to obtain the subject space weights in a simultaneous comparison of all groups under consideration. Table 6 shows the subject weights for the three-dimensional solution (VAF = .87) for the six data sets. The input data were the scaled metric distances from TORSCA, and not raw test data.

From the analysis of stress figures reported earlier, we were confident in inferring that the *WA and RSref data sets were both two dimensional and that the *RD and the remainder of the data sets were three dimensional. We performed a three-dimensional INDSCAL analysis because that dimensionality provides a direct test of variations among the data sets: the *WA and RSref input matrices should show very low or zero weight in one of the three dimensions. This outcome is apparent in that the second dimension of *WA has a weight of zero and the RSref a weight of .19. In addition, an examination of Table 6 shows that the pattern of weights of these two imput matrices is uniquely different from that of the other four matrices.

It will also be useful to examine the INDSCAL group stimulus space for a comparison of the structures of the eight kinship terms in each input matrix. Figures 2, 3, and 4 present the three-dimensional picture, two dimensions at a time. To obtain the structure for a particular matrix, it is necessary to utilize the weights given in Table 6. For the *WA and RSref matrices, this means that their structures include only dimensions one and three, which are shown in Figure 3. The arrangement of terms in Figure 3 closely resembles that of the dimensional values given by the Wallace-Atkins structural model, as shown in Table 3. The other two figures, 2 and 4, can best be interpreted in terms of the dimensional values of the Romney-D'Andrade structural model, as shown in Table 4.

To summarize, the three-way scaling analysis confirms the results of the two-way scaling analysis. The referenced and unreferenced rating-scale test data, that is, RSref and RSunref, correspond to the predicted structural models, those of Wallace-Atkins and Romney-D'Andrade respectively, whereas the referenced and unreferenced triads test data, that is, Tref and Tunref, both have underlying structures similar only to the Romney-D'Andrade model.

TABLE 6. Three-Dimensional INDSCAL Weights for Four Data Sets and Two Process Models.

		*WA	RSref	*RD	RSunref	Tref	Tunref
	1	.77	.59	.43	.41	.33	.25
Dimensions	2	.00	.19	.48	.53	.62	.57
	3	.49	.51	.60	.56	.57	.63

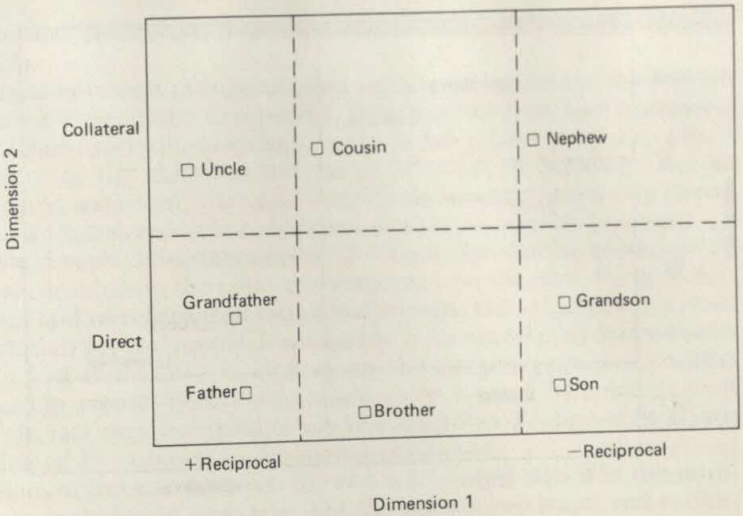

FIGURE 2. Group stimulus space showing dimension one and dimension two of the three-dimensional representation.

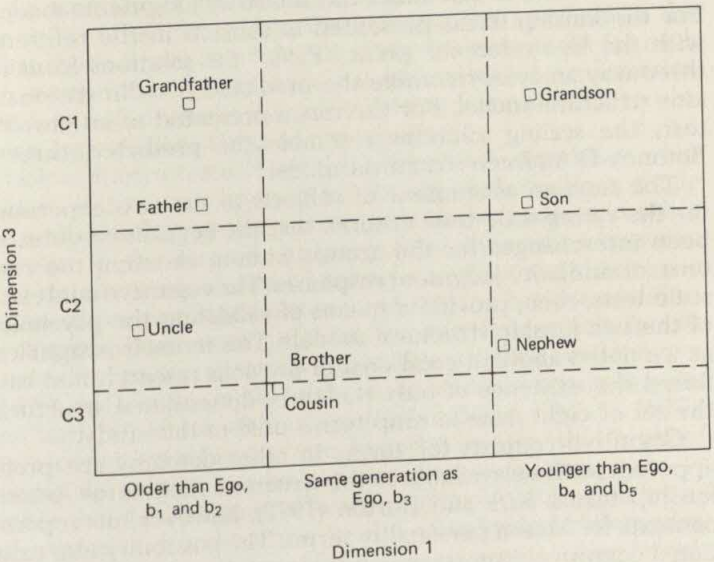

FIGURE 3. Group stimulus space showing dimension one and dimension three of the three-dimensional representation.

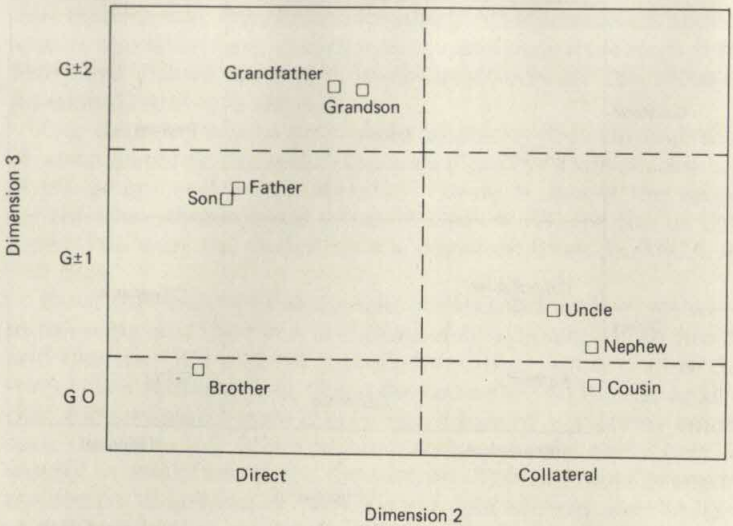

FIGURE 4. Group stimulus space showing dimension two and dimension three of the three-dimensional representation.

CONCLUSION

The results of the multidimensional scaling analysis of *rating-scale dyads data* on American English kinship terms demonstrate that varying the cognitive contexts can affect the similarity judgments made by subjects. For the kinship terms presented to subjects in the referenced context, with the Ego reference point "Pat's," the solutions from the two- and three-way analyses resemble the predicted two-dimensional Wallace-Atkins structural model. For the terms presented in an unreferenced context, the scaling solutions resemble the predicted three-dimensional Romney-D'Andrade structural model.

The random assignment of subjects to the two experimental groups for the rating-scale tests ensures that the cognitive contexts could have been interchanged for the groups without affecting the respective patterns of similarity-judgment responses. The cognitive contexts with rating-scale tests, then, provide a means of validating the psychological reality of the two kinship structural models. This in itself is significant because, as we noted above, a good deal of previous research had basically established the existence of only the three-dimensional structural model for the set of eight male kinship terms used in this study.

Cognitive contexts for terms in other domains are probably not so apparent as those available in the structural models for American English kinship terms. Kirk and Burton (1977), however, do report on a set of contexts for Maasai personality terms. The possibility also exists that additional cognitive contexts may be associated with the domain of American English kinship, inasmuch as there have been other formal analyses of

the domain that differ from those used in this study (for example, Goodenough 1965).

Differences between rating-scale and triads test results for referenced kinship terms were quite unexpected. Researchers who had compared these and other similarity-judgment methods have been emphatic about the reliability of the different tests as predicators of behavior. But as far as we could ascertain, the various methods have not been compared in terms of an intervening variable such as that of cognitive context.

Why would such differences occur? Perhaps this can be answered in part by first considering the pattern of responses on the tests. For unreferenced triads and unreferenced rating-scale dyads, the responses matched the predicted structural model. It was on the referenced tests that subjects apparently had difficulties in identifying the proper cognitive context for the kinship terms. Referenced triads were treated by subjects as if they were in fact unreferenced, while the referenced rating-scale dyads were evaluated by subjects as the model predicted.

An obvious difference between the two referenced tests is in the number of words included in each trial. For the referenced triads and rating-scale dyads, respectively, there are six and four words used. There may be a certain threshold of terms per trial beyond which subjects can no longer keep a referenced context separate from an unreferenced. Whether this can indeed account for the differences between expected and actual results was not determined in the experiments, but it certainly appears to merit further investigation.

The existence of alternative cognitive structures has been the central issue in the research reported here. We have shown that they do exist with respect to American English kinship terms. But the current paradigm for the study of semantic domains seems to be based upon the assumption that there is one and only one underlying structure for each domain. This assumption may have led to situations in which the detection of evidence in favor of alternative cognitive structures was hindered by the researcher's expectations of his or her results.

The main task of future research on this subject is to determine what kind of criterion, if any, can be used to distinguish between a single structure and multiple alternative structures for the same items in other semantic domains. In the research reported here, differences between the underlying structure of referenced rating-scale data and that of the data for the other three test conditions seemed striking because of differences in dimensionality and in configuration. But even if structures appear to share the same dimensions, alternative cognitive structures with unique patterns of INDSCAL weights might be shown to be differentially correlated with some variable factor such as age (Howard and Howard 1977).

If alternative cognitive structures are hypothesized for the items of a given semantic domain, as we have done here, it is appropriate to consider whether the use of these structures operates for subjects in terms of a model of cognitive pluralism or a model of individual differences. We conclude that the appropriateness of the cognitive contexts for American English kinship terms analyzed in this paper make it highly likely

that the same individuals could in fact interpret at least these terms, if not also those constituting other semantic domains, in conformity with a model of cognitive pluralism.

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Synchronic Variability and Diachronic Change in Basic Color Terms[1]

PAUL KAY*†

The main purpose of this paper is to relate recent findings regarding the evolution of basic color term systems to current research in linguistic variation and language change. More specifically it is to provide in the area of lexical semantics a demonstration of the hypothesis that all linguistic change has its roots in synchronic heterogeneity of the speech community (cf. Weinreich, Labov & Herzog (1968:188) for a general statement of this position, also many empirical demonstrations already reported, such as those by Labov (1972) and Wang (1969) in phonology and by Bickerton (1973) in syntax). Before taking up this topic, I summarize some recent revisions in the basic color term theory itself.

[1] I am indebted for helpful comments on various drafts of this paper to Brent Berlin, Janet Dougherty, John Gumperz, Dell Hymes, William Geoghegan, Chad McDaniel, and Judith Shapiro, none of whom is responsible for any of its faults. Earlier versions of this paper were read in Fall 1974 to the Linguistics and Social Anthropology Groups at Berkeley and at the Annual Meetings of the American Anthropological Association in Mexico, D.F. I am grateful for many useful comments from the floor at these sessions.

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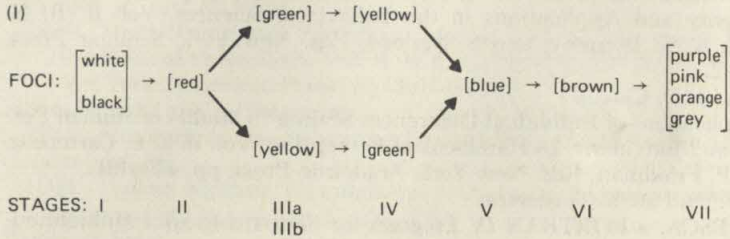
† Source: *Language in Society* 4:257–270, 1975.

REVISIONS OF THE BASIC COLOR TERM THEORY

The major data on which the revisions of the basic color term theory are based are contained in four detailed studies of the color term systems of individual speech communities which have been undertaken since 1969 (Berlin & Berlin 1975; Dougherty 1974; Hage & Hawkes 1975; E. Heider 1972*a, b*). Several other reports, cited below, have been influential as have a number of personal communications from scholars familiar with a variety of languages.



The original Berlin and Kay hypothesis was that the basic color terms of all languages encode some subset of a set of eleven fixed perceptual foci and that there is a partially fixed temporal order in which these foci are encoded, shown in (1).

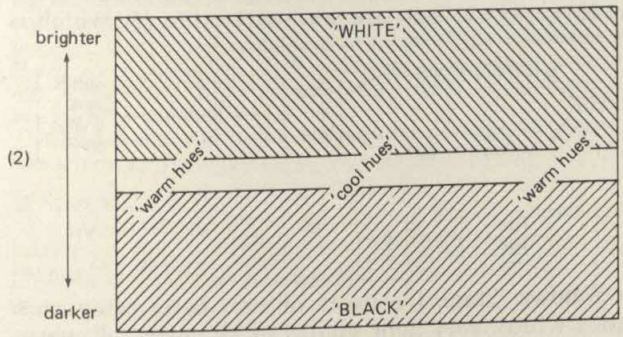
The new data suggest that both the sequence itself and its interpretation need revision. In the original report (Berlin & Kay 1969) while sequence (1) was interpreted as representing the successive encoding in basic color terms of perceptual foci, the discussion of the sequence considered not only the encoding of foci but also the gradual restriction of boundaries as the sequence progresses. It now appears that not only must the sequence itself be slightly altered but more importantly its interpretation must be altered to encompass not only the encoding of new foci but the subtle interaction of foci and boundaries of color categories.

E. Heider (1972*a, b*) was the first person since 1969 to study the color words of a large number of informants from a speech community whose language, considered as a homogeneous whole, constitutes an early system—the Dugum Dani of New Guinea, stage I. She found that there were indeed just two color terms used reliably by all informants and that these might reasonably be glossed as black and white or dark and light—as reported by K. Heider (1965) and later by Berlin & Kay (1969). She also found, however, two facts not envisaged by either K. Heider or Berlin and Kay but subsequently confirmed in other controlled studies of simple systems: *(a)* the dark term, *mili* includes cool hues (blues and greens) while the light term *mola* includes warm colors (reds, yellows, browns, purples, pinks) so that the boundary between *mili* and *mola* is roughly that shown in (3) rather than that hypothesized in Berlin & Kay (1969:17) and reproduced as (2); *(b)* the focus of these terms is quite variable across informants and in fact the most popular focus for *mola* is not in white but in red. Sixty-nine per cent of Dani focal responses for *mola* were in or near English focal red. It is thus inaccurate to say

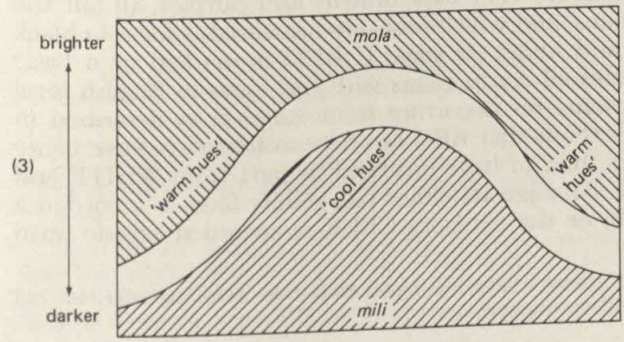
that two-term systems make a simple brightness contrast. Rather such systems contrast dark and cool hues on the one hand against light and warm hues on the other. Within these categories it appears that foci may be variable across informants and across languages, about which more below. This reformulation is supported by recent data on other early systems which limitations of time prevent my discussing here (cf. Berlin & Berlin 1975; Hage & Hawkes 1975; Dougherty 1974). It is also confirmed by previously existing data such as Conklin's on Hanunóo (1955), a stage IIIa system in which the black term, *-biru*, includes blue and appears to be a reflex of a Proto-Polynesian root meaning blue, $*bi[l]u^c$ (Dempwolff 1937:27).

The above points affect only the interpretation of sequence (1) but not the sequence itself. There is a further point involving blue that affects the interpretation of the sequence in the same way and also the ordering.

Berlin & Kay (1969:42) noted that Japanese *ao* "blue" appears on the basis of internal reconstruction to be older than *midori* "green," and that moreover there is evidence that *ao* once included greens as well as blues. This suggested that at an earlier stage Japanese would have had a GRUE term *(ao)* with the focus in blue and would thus have violated the hypothesized sequence of encoding of foci by having encoded blue before green. We were not then willing to abandon sequence (1), which



'WHITE'/'BLACK' color system as hypothesized by Berlin and Kay (1969)

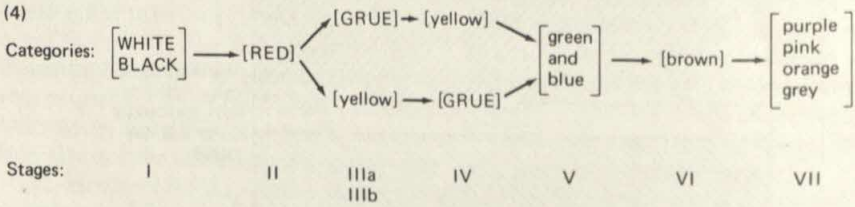


An actual 'WHITE'/'BLACK' color system as reported by E. Heider for the Dani
mola = 'WHITE'
mili = 'BLACK'

specified green before blue, on the basis of this single inferential counter-example. Since that time, however, further cases in which blue is encoded before green or contemporaneously with green have come to light. Berlin & Berlin (1975) report that Aguaruna *wíŋka* GRUE has virtually all (97 per cent) of its focal responses in blue. In addition it may be recalled that Berlin & Kay (1969:10f.) report that approximately one-fourth of forty Tzeltal informants placed focal *yaš* in blue; similarly Heinrich (1973) indicates that Eskimo *tungu-* GRUE is focused in both green and blue (never in blue-green) with some (most?) informants showing a preference for blue. Dougherty (1974) reports Futunese *wíwi* GRUE focused in both green and blue, preponderantly the latter.[2] As these authors point out, the new data require a revision of the theory to allow the blue focus to be encoded before or simultaneously with the green focus. To be sure, as Berlin and Berlin indicate there are no data suggesting that both green and blue are ever encoded before yellow; that is, there are no basic color systems with black, white, red, green and blue foci encoded although there are systems with black, white, red, yellow and blue foci encoded and probably systems with black, white, red, yellow and green foci.

These facts about color foci have an important implication, namely that the operative element in the sequence at state III is neither the focus green nor the focus blue but the *category* GRUE. That is, the category GRUE may be accorded a basic color term either before or after the yellow focus is encoded, but GRUE is never split into green and blue and labeled with two basic color terms until after the yellow focus is named at the basic level. These facts are summarized in (4), which is

(4)



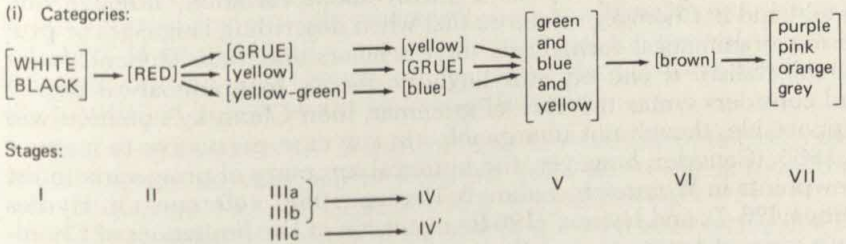
to be interpreted as follows: Stage I consists of two basic categories: WHITE, which includes white, very light shades of all colors, all warm colors, and may have its focus in either white, red, or pink and BLACK which includes black, some very dark browns and purples, all but the lightest blues and greens, and which probably has variable focus in black and in dark greens and blues. At stage II RED is marked by a basic color term and includes all warm colors with the focus in English focal red; this stage represents no departure from stage II as described in Berlin & Kay (1969) except that RED can be considered to have come out of WHITE rather than to have emerged in part from WHITE and in part from BLACK. At stage III either the yellow focus is accorded a basic color term (IIIb) or the category GRUE is accorded a basic term

[2] D. Snow (1971) and H. Broch (1974) also report basic color term status for blue but not green in Samoan and Slavy respectively.

(IIIa). In the latter case the focus may be either in blue or in green or perhaps in both, but there is no evidence that in any language the focus of GRUE is in what we would call "blue-green."[3] At stage IV stage IIIa systems add yellow and stage IIIb systems add GRUE, so that all stage IV systems contain terms for WHITE, BLACK, RED, GRUE, and yellow, with foci as described for stage III. Then at stage V, GRUE is split into green and blue. Stages VI and VII remain unchanged from Berlin & Kay (1969) with the following reservation. Berlin & Kay (1969:42ff.) cited the premature appearance of grey in two Uto-Aztecan languages (Hopi and Papago); since then, this has been confirmed for other Uto-Aztecan languages, reconstructed for proto-Uto-Aztecan by Hill & Hill (1970) and corroborated by Nichols (1974:250–81). Grey is also prematurely present in Samal (Austonesian, Philippines) as noted by Geoghegan (personal com-

[3] A third possibility, at present remote, suggests itself for stage III. The Hanunóo basic terms (Conklin 1955), Dani secondary terms (E. Heider 1972*b*) and some fragmentary data from several Miwok languages (E. Callahan, personal communication) suggest that possibly at this stage a yellow focus may be encoded which extends over the greens. If this does indeed happen sometimes at stage III, then one would expect at stage IV for a blue term to emerge with the yellow-green category still focused in yellow and then for the latter yellow-green to split at stage V producing a standard stage V system. The sequence would then be generalized as follows to account for these cases. M. Haas reports that Creek, a Muskogean language, has (or had) basic terms for white, black, red, yellow-green, and blue; thus Creek would have a stage IV′ system (personal communication). Professor Haas also reports that Natchez, a language—now dead—that may have been a distant relative of the Muskogean family, had basic terms for white, black, red, yellow-green, blue and grey. Natchez would then have had a stage IV′ system also, as well as showing premature grey (cf. above). The remaining languages of the southeastern United States on which Haas reports, including all the members of the Muskogean family and some languages not in this family, have 'normal' color systems, i.e., contain either a term for GRUE or one each for blue and green.

Haas has also called to my attention the possibility that Proto-Indo-European may have had a term for yellow-green.



. . . the reconstruction for both [yellow and green, P.K.] is *ĝhel- and our word yellow comes from this. This information comes from Buck (1949). . . . Buck was not thinking in terms of a single term for yellow-green, but on p. 1058 he speaks of 'IE *ĝhel- in words for bright, shining colors, esp. "yellow, green," also "white" and "blue." ' And on p. 1059 he says 'A considerable group of the words for "yellow" [in the daughter languages, M.H.] are cognate within others for "green" ' (M. Haas, personal communication).

These data are suggestive. We have not yet, however, had a report of a language of stage IIIc (or IV′) where the explicit claim is made that the signification of the color terms has been determined by systematic use of controlled stimulus materials. Until a report of this kind is in hand, stages IIIc and IV′ remain interesting but unproven possibilities.

munication) and reported in Berlin & Kay (1969:44), in Yir-Yoront [Australia] (B. Alpher, personal communication), in several Athabascan languages (Durbin 1974), in Zapotec (R. MacLaury, personal communication) and in Natchez (M. Haas, personal communication). Thus the possibility mentioned by Berlin & Kay (1969:45) that grey may appear as a wild card is supported by new data.

In sum, the major revisions of the theory are (a) recognition of the integrity of the light-warm, dark-cool, and GRUE categories in early systems and (b) recognition of the role played by category boundaries as well as foci in the evolutionary sequence.

RELATION TO CURRENT RESEARCH ON LINGUISTIC VARIATION AND LANGUAGE CHANGE

The original basic color term theory was conceived within the received tradition that considers the proper object of linguistic study to be Saussurian *langue* or Chomskian linguistic competence. (The *langue/parole* and linguistic competence/linguistic performance distinctions are essentially the same, except that Chomsky admits sentential structure to competence while de Saussure exiled it to *parole*. Also, of course, Chomsky emphasizes the psychological aspect of language while de Saussure emphasized the social.) According to Chomsky

> Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech community, who knows its language perfectly . . . This seems to me to have been the position of the founders of modern general linguistics, and no cogent reason for modifying it has been offered (1965:3f.).

One might argue whether this was in fact the position of the men to whom Chomsky was referring (whoever they might be). Sapir and Bloomfield, for example, both spoke explicitly about variation, although one should add in Chomsky's defense that when describing languages or proposing grammatical formalisms these scholars and their students were monolectalists. If one equates linguistic theory to grammatical theory and considers syntax the core of grammar, then Chomsky's position was supportable, though not unarguable—in any case persuasive to many—in 1965. (Consider, however, the historical summary of proto-variationist viewpoints in Weinreich, Labov & Herzog (1968), references in Hymes (1964a:426–7), and Hymes' (1964b) discussion of the limitations of Chomsky's view of language and their historical antecedents. I am indebted to John Gumperz and Dell Hymes for illuminating discussions of these issues.)

But since the mid-sixties, the "homogeneous speech community" has been found to be not just an idealization but a myth. Empirical studies of changes in progress due principally to the influence of Labov, have demonstrated repeatedly that linguistic change has its source in synchronic variation, that is, in heterogeneity of the speech community. In the words of Weinreich, Labov and Herzog, "Not all variability and

heterogeneity in language structure involves change; but all change involves variability and heterogeneity" (1968:188). The doctrine that diachronic change implies synchronic variation has received numerous convincing demonstrations, e.g., by Labov (1972) and Wang (1969) in phonology and by Bickerton (1973) in syntax (see also the various articles in Bailey & Shuy (1974)). Linguistic change has been shown to result from the redistribution of synchronic linguistic variants in a population across speakers, styles, and linguistic environments. New variants replace old ones neither instantaneously nor so gradually as to be unobservable. An excellent and very early statement of this principle was made by Vogt:

> At any moment, between the initiation and the conclusion of these changes, we have a state characterized by the presence of more or less free variants . . . What therefore in a history of linguistic system appears as a change will in a synchronic description appear as a more or less free variation . . . (Vogt (1954:367) quoted in Wang (1969:16)).

To this must be added the recent finding that linguistically "free" variants involved in changes are most often socially conditioned as well as being subject to types of linguistic conditioning not hitherto countenanced by linguistic theory, e.g., "natural conditioning"—Stampe (1972), quantitative conditioning—Labov (1969), and lexical conditioning—Wang (1969).

What are the implications of this development in the general theory of linguistic change for our little theory of evolution of basic color term systems? First, we can predict in a broad way that in a language in which the color term system is undergoing change there will be inter-speaker variation. Secondly, since not all aspects of a language change at the same time (for example the vowel system may be changing at a time when the consonantal system is relatively stable; cf. Labov (1972:517)), we will expect some languages to show minimal inter-speaker variation in color terminologies while others will show considerable inter-speaker variation. More specifically, within the context of the general theory of linguistic variation and change, we may make the following predictions about variation in synchronic basic color lexicons.

(5) a. If all speakers are at a given stage n with respect to basic color terms, the most salient secondary color terms will be those that become basic at stages $n + 1$, $n + 2$, and so on.

b. The relative degrees of salience of the secondary terms should, in so far as they may be assessed, follow the ordering of the predicted evolutionary sequence (4). (For example, if all informants are at stage I with respect to basic terms we would expect the most salient secondary term, if there is any, to be RED, the next most salient to be either GRUE or yellow, and so on.)

c. In a speech community undergoing change in basic color lexicon, not all speakers will be at the same stage with respect to basic color lexicon, but each speaker will be classifiable as to stage.

d. In a community undergoing change in basic color lexicon, the totality of stages represented will be contiguous in the sequence (4).
e. In a community undergoing change in basic color lexicon, difficulties in classifying speakers as to stage will involve only stages adjacent in the sequence (4).
f. In a community undergoing change in basic color lexicon the basic color terms added at later stages are present as secondary terms for speakers at earlier stages.
g. In a community undergoing change in basic color lexicon, stage of speaker will correlate with various social factors depending on the local social situation, but there should be a pervasive correlation with age (cf. Labov 1972:519 ff.).

Berlin investigated in detail forty Tzeltal speakers and found that all informants had stage IV basic color lexicons in Tzeltal despite the fact that these informants represented the full range from monolingualism to complete Tzeltal–Spanish bilingualism (Berlin & Kay 1969:10, 31 ff.). This contrasts with the Aguaruna, Binumarien and West Futuna cases to be discussed below, each of which has speakers at stages III, IV, and V. This observation confirms our first general prediction: that some early color systems will be diachronically relatively stable and show minimal synchronic heterogenity while others in the process of change will show considerable inter-speaker variation.

The data most directly relevant to predictions (5) a and b are those of E. Heider (1972*a*, *b*) from the Dugum Dani, a non-Austronesian language of Highland New Guinea. Heider reports that "the language . . . lacks basic chromatic color terms" (1972*a*:12). "There were only two color terms (*mili* and *mola*) which were used by all informants" (1972*b*:451). These may be glossed roughly as BLACK and WHITE bearing in mind the fact that the majority of informants picked focal red as the focus for the term we gloss WHITE. Heider goes on to report an "unexpected" finding that 50 per cent of her informants ($n = 40$) had a term for red, 45 per cent for yellow and 28 per cent for blue. These glosses are supported by data from term to stimulus mappings for forty informants on a color chip array constructed by taking every other hue for all brightnesses of the array used by Berlin and Kay. These secondary foci are predicted by sequence (4) and the degrees of salience, as evidenced by the number of speakers using the term, also fit the sequence. Thus predictions (5) a and b are confirmed in these data. The Dani data do not offer evidence pro or con regarding the other predictions (5) c-f. Prediction (5) g will be considered below.

Berlin and Berlin have not tabulated incidence or any other index of salience for Aguaruna secondary color terms. (Aguaruna is a Jivaroan language of Eastern Peru.) For their stage III informants they report secondary terms, some of quite variable focus, that range over yellow, yellow-green, green, brown, and purple as well as a term which literally means "ripe," is of very broad reference, and is probably not truly a

color term at all. These facts accord with prediction (5) a. It is B. Berlin's impression that the purple term is better established than the brown term as regards both the frequency with which it is offered by informants and the narrowness of its focus and range (personal communication). This datum controverts prediction (5) b. Apparently (5) b is a prediction that we may expect to be borne out on the average, but not necessarily in each particular case. The fifty-five Aguaruna informants tested by Berlin and Berlin displayed basic color term systems corresponding to stages IIIa (thirty-four informants), IV (five informants) and V (sixteen informants). Predictions (5) c and (5) d are thus confirmed. Berlin and Berlin do not discuss in their paper problems of assigning informants to stages, but B. Berlin reports that such problems as did arise always involved the assignment of the informant to one of two adjacent stages (personal communication); this report supports prediction (5) e. Prediction (5) f is also borne out by the Aguaruna data: all basic color terms which are added to the stage IIIa set by stage IV and stage V informants occur frequently as secondary color terms for stage IIIa informants. Discussion of prediction (5) g will be postponed until after the Binumarien and Futunese data are discussed individually.

Binumarien is a non-Austronesian language of Highland New Guinea. Of the thirty-six Binumarien stage IIIb informants, twenty-one possess a secondary term for green, sixteen one for blue, and nine possess one for both. There are no other secondary terms, all other low frequency color terms being synonyms for basic color terms. Since the stage IV informants of this language show a preference for placing the GRUE focus in green, these statistics accord perfectly with predictions (5) a, (5) b and (5) f. Predictions (5) c and (5) d are confirmed in that there are thirty-six informants at stage IIIb, eight informants at stage IV and one informant (a fourteen-year-old) at stage V. Two informants are unclassifiable as between stages IIIb and IV as it was impossible to determine for them whether the GRUE category was contained in BLACK, which would make them IIIb, or whether their GRUE contrasted with BLACK, which would make them stage IV. This is a typical sort of classification problem, which confirms prediction (5) e and supports the evolutionary sequence. (Hage and Hawkes report a single informant whose classification represents a problem to the sequence as a whole but only in a quite minor respect. This nine-year-old has a normal stage IV system except that she includes purple in the range of GRUE.)

The data from West Futuna, a Polynesian outlier language, reported by Dougherty also confirm all predictions except (5) b, for which there are no relevant data. Stage IIIa speakers have as secondary terms the two terms for yellow that emerge at stage IV as basic terms. The same is true for stage IIIa and IV informants with respect to English *blue*, which becomes a basic term at stage V. (A single informant of the forty with a basic term for yellow uses Aniwan *féroféro* for yellow, a term otherwise unattested for West Futuna. The remainder use either *hléo* or *yéla* both of which occur frequently as secondary terms for stage III informants.) Predictions (5) a and f are thus confirmed. There are no

Futunese data relevant to prediction (5) b. Predictions (5) c and (5) d are confirmed by the fact that all informants are classifiable as stage IIIa (five informants), IV (eighteen informants), or V (twenty-two informants). Prediction (5) e is confirmed by the fact that there are several interesting problems regarding classification of informants as stage IIIa versus stage IV and also as between stages IV and V but none in assigning an informant to at least one stage (the reader is referred to Dougherty's paper for details).

We now consider prediction (5) g regarding the social correlates of evolutionary stage of basic color nomenclature. As predicted, we find varying social correlates depending on local conditions. Berlin and Berlin report that for Aguaruna speakers contact with Hispanic culture in general and degree of proficiency in the Spanish language in particular are the major correlates of and causes of movement along the color term sequence. For Binumarien, Hage and Hawkes report that males are more familiar with Neo-Melanesian than females; seven of the eight Binumarien informants with stage IV or V systems are males. While the Aguaruna and Binumarien data support the hypothesis of language contact as a source of innovation in color term evolution, the West Futuna data controvert this hypothesis. Dougherty reports that her stage V informants are almost all women, and that women are on the average less traveled than men and have less knowledge of Neo-Melanesian and other foreign languages. Innovativeness in-color lexicon in West Futuna seems negatively correlated with degree of exposure to more advanced color systems.

With regard to age, we find the predicted pervasive correlation between age and color stage in those cases where data are available. The Dani data are equivocal. E. Heider did not make age estimates but split her sample into "adults" and "children." She reports that adults did not use more chromatic terms than children. She does not say whether children used significantly more chromatic terms than adults. Caution suggests that we assume from the lack of explicit statement to the contrary that Dani children do not use significantly more chromatic terms than Dani adults. It has, however, been observed that children acquire full knowledge of their color lexicon relatively late with respect to many other aspects of language (Istomina 1963; Hoppman n.d.; Hage & Hawkes 1975). This poses a problem for the comparison of matched child and adult samples for color terminology systems; there is no way of knowing whether the number of terms controlled by children is artificially lowered because the children have not yet learned all the color terms they will eventually learn. Thus it could well be the case that if and Dani who are now children use about the same number of chromatic terms as members of their parents' generation, they may nevertheless eventually use more chromatic terms than their parents. To sum up, the Dani data as reported are unclear on the issue of a correlation between age and use of color terms.

The reports of the remaining three speech communities offer age estimates for each informant. Hage and Hawkes do not discuss the operational basis of their age variable, but Berlin & Berlin (personal communication)

and Dougherty (personal communication) emphasize that their informants do not keep track of their ages and that the age estimates are consequently very rough. Internal evidence in the Hage and Hawkes data, consisting of a disproportionate number of ages that end in the digits zero and five, as well as general ethnographic knowledge of Highland New Guinea, suggest that the Binumarien age data also represent rather wild guesses. A further complication is that the Binumarien data include young children while the Aguaruna and Futunese data exclude young children. Binumarien informants under the age of fifteen have consequently been excluded from the following calculations.

As noted, the age data to be analyzed are subject to error from a large number of undisclosed sources. Unless the overall effect of these various, presumably uncorrelated, sources of error happens accidentally to correlate with development of color lexicon, we should expect correlations between age of speaker and stage of color lexicon to be artificially depressed. That is, the most cautious expectation would be that correlations reported below *underestimate* those actually existing in the populations under study.

Bearing in mind these caveats, we consider the correlations between age and stage of basic color lexicon shown in Table 1. The pervasive correlation predicted in (5) g in fact appears. T-tests for differences in mean ages for informants of color terms stages III, IV, and V show the following results. Aguaruna stage V speakers are significantly younger than stage III speakers and also significantly younger than stage IV speakers. Stage IV and V speakers combined are significantly younger than stage III speakers. There is no significant difference between stage IV and stage V speakers.

For Futunese we find that stage V speakers are significantly younger than stage IV speakers and also significantly younger than stage III speakers. Stage V speakers are significantly younger than those of stages III and IV combined. The Futunese data contain the only reversal in the absolute predicted order of mean ages to be found among these groups. Futunese stage IV speakers are slightly older than stage III speakers, but the difference is not statistically significant and so may be attributed to sampling error.

For Binumarien, Hage and Hawkes report accurately that stage IV and V systems "are possessed exclusively by a subset of younger members of the population." The range of ages of stage IV and V informants is from eight to eighteen years. The mean ages conform to the predicted sequence; earlier stage informants are older than later stage informants. There is only one informant at stage V and this is a fourteen year old. Stage IV informants are significantly younger than those of stage III. (Informants of stages IV and V combined are also significantly younger than those of stage III, but the stage V data are not shown in Table 1 because all stage V speakers are juveniles, i.e. younger than fifteen years old.)

All the significant differences point in the same direction—that younger speakers have more advanced basic color term systems than older speak-

TABLE 1. Significant Differences in Mean Ages of Adult Speakers of Basic Color Term Stages III, IV, and V in Aguaruna, Futunese, and Binumarien

Language	Stage	Number of speakers	Mean age	Standard deviation of ages	Significant differences in mean age between speakers of different stages	Level of significance attained (one-tailed t-test of difference between independent means)
Aguaruna	IIIa	34	33.32	12.84		
	IV	5	22.30	14.04		
	V	16	21.44	9.80		
					$\overline{\text{III}}-\overline{\text{IV}} = 11.02$	$P(t_{37} \geq 1.77) < 0.05$
					$\overline{\text{III}}-\overline{\text{V}} = 11.88$	$P(t_{48} \geq 3.27) < 0.005$
					$\overline{\text{III}}-(\overline{\text{IV}} \cup \overline{\text{V}}) = 11.88$	$P(t_{53} \geq 3.50) < 0.0005$
Futuna	IIIa	5	45.00	15.41		
	IV	17	48.32	16.05		
	V	21	35.14	10.79		
					$\overline{\text{III}}-\overline{\text{V}} = 9.86$	$P(t_{24} \geq 1.70) < 0.05$
					$\overline{\text{IV}}-\overline{\text{V}} = 14.41$	$P(t_{37} \geq 3.43) < 0.005$
					$(\overline{\text{III}} \cup \overline{\text{IV}})-\text{V} = 12.81$	$P(t_{41} \geq 3.11) < 0.005$
Binumarien	IIIb	19	41.32	16.33		
	IV	6	27.83	3.49		
					$\text{III}-\text{IV} = 13.49$	$P(t_{23} \geq 1.99) < 0.05$

ers. Considering the approximate nature of these age estimates, a statistical pattern as consistent as the one we have found would seem to support strongly prediction (5) g.[4]

SUMMARY

We have derived predictions about synchronic variability in basic color term systems from the interaction of the general postulate that diachronic change always implies synchronic heterogeneity in the speech community with out knowledge of the results of linguistic change in basic color lexicons embodied in the evolutionary sequence (4). Testing these predictions against the available data on the distribution of color lexicons in actual speech communities, we have found the predictions confirmed. Speech communities may contain speakers of several adjacent evolutionary stages. Individual speakers are virtually never hard to place in the sequence, but sometimes hard to place in one of two adjacent stages. The pattern of elevation of secondary terms to primary terms follows the predicted sequence. Stage of basic color lexicon correlates with a variety of social indices in different societies, but there is an overall tendency for speakers with more advanced basic color term systems to be younger. We thus find confirmation in the domain of color words for the doctrine that linguistic change implies synchronic heterogeneity in the speech community.

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[4] All the tests of significance performed are technically based on the assumption that informants were sampled randomly from their respective populations. This assumption is almost surely violated in all three languages, as the samples were probably drawn more on the basis of convenience than on any other. It is possible that the ethnographers' convenience systematically selected informants in such a way as to exaggerate the correlation of age and basic color term stage, but since this seems *a priori* quite implausible the burden of proof would appear to be on whoever might wish to make this argument.

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15

Cognitive Maps of the Ethnic Domain in Urban Ghana: Reflections on Variability and Change

ROGER SANJEK*†

A concern with variation *among* cultures lies at the very foundation of the discipline of anthropology. Ethnographers have long been occupied with describing how different societies, each in its own unique way, go about such universal tasks as raising children, obtaining food, and categorizing domains of experience. From ethnographic descriptions anthropologists then attempt to identify similarities, differences, and patterned covariation among cultures in order to formulate sociocultural theory.

In recent discussions many anthropologists have stressed the importance of variability *within* cultures as well (see Pelto and Pelto 1975 for a programmatic statement of this orientation). The focus upon intracultural variability or diversity has two critical components. The first is a desire to raise the standards of ethnographic description so that similarities, differences, and patterned covariation may be more clearly identified, and more comprehensive sociocultural theory might result. The second aspect of the concern with intracultural diversity is the development of theory from *within* particular societies themselves. The identification of similarities, differences, and patterned covariation within a culture (however bounded) can lead to the formulation of theory of a very different sort from theory that begins with "whole" cultures as units of comparison.

It is toward such single society theory—the identification of similarities, differences, and patterned covariation in cognition and behavior (and the relations among them) within one culture, and the development of hypotheses that may be tested within other cultures—that this paper is addressed.[1] There is little new about such an enterprise in anthropology aside from the thoroughness with which ethnographers attuned to intracultural variability are going about it. This thoroughness is especially evident in recent work on cognitive variability (Berlin and Berlin 1975; Goodenough 1971; Hammel 1970; Heider 1972; Howe and Sherzer 1975; Johnson 1974; Keesing 1974; LeVine and Price-Williams 1974; Micklin, Durbin, and Leon 1974; Roberts, Chiao, and Pandey 1975; Sanjek 1971; Sankoff 1971).

In this paper I shall use some of the approaches of what is variously called the new ethnography, ethnoscience, or cognitive anthropology to examine the domain of ethnic identities among a polyethnic West African population. I first provide a brief account of Adabraka, the setting in which data were collected. Then I describe the content and scope of the domain of ethnic identities, or "tribes," from the viewpoint of

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Adabraka people, and I explain how a single implicit structure of language group distinctions underlies a variety of explicit individual modes of organization of the domain.

In the central portion of the paper I turn to the comparison of cognition and behavior. I show that there is a close, almost one-to-one, correspondence between the appearance of individuals of particular ethnic identities in sample sequences of everyday interaction and the cognitive salience of these identities in free recall. The same close relationship is seen to exist between behavioral sequences and the underlying structure of language group distinctions.

The importance of behavioral experience is stressed again in the final section, which examines processes of change in the domain of ethnic identities. An argument is also made here that state policy must be reckoned with as a source of cognitive change "from above." I show how versions of the domain of ethnic identities promulgated by the Ghanaian state are today of coordinate importance with behavioral experience "on the ground." In conclusion, I formulate from the basis of my analysis a logical series of points at which cognitive diversity may be investigated in research oriented to current and changing relationships between cognition and behavioral experience.

THE SETTING

Ethnographic data on cognitive and behavioral aspects of ethnicity were collected during research in Accra, Ghana in 1969–1971 and 1974.[2] Accra in 1970 was a city of almost 650,000; its population comprised more than eighty ethnic identities.[3] The base from which fieldwork was conducted was eleven apartment buildings facing onto one street in the Adabraka section of Accra. The 423 residents of these buildings used twenty-three different ethnic labels to identify themselves.

Several of these ethnic groups were considered to speak mutually intelligible dialects of either Adangbe or Twi, but nonetheless fourteen different languages were spoken by the neighborhood residents as first languages. Table 1 lists these ethnic identities and languages.

The extent of behavioral mixing among individuals of different ethnic identities was considerable. Common use of the building courtyards provided a major focus for interethnic contacts among residents; the number of apartments rented to tenants per building ranged from one to sixteen, with the number of different ethnic identities per building ranging correspondingly from two (in the smallest building) to twelve. Nineteen percent (23/124) of the individual households were ethnically mixed, and 37 percent (46/125) of the adults who had ever been married had been partners in interethnic matches. Both casual participant observation and systematic behavioral data (to be discussed below) indicated that daily interaction sequences for these Adabraka residents tended to be polyethnic more often than not. Other recent studies in urban Ghana support the Adabraka findings of residential and behavioral ethnic mixing (Grindal 1973; Owusu 1970:92–116; Peil 1972:155; Quarcoo, Addo, and Peil 1967:88; Schildkrout 1974).

TABLE 1. Ethnic Composition of the Adabraka Population (N=423).

Ethnic Identity	Language	Number	Percentage
Kwawu	Twi	109	24.8
Ga	Ga	74	17.5
Ewe	Ewe	67	15.8
Nzima	Nzima	26	6.1
Akwapim	Twi or Kyerepong	25	5.9
Fanti	Fanti	24	5.7
Ashanti	Twi	19	4.5
Yoruba	Yoruba	18	4.3
Hausa	Hausa	10	2.4
Krobo	Adangbe	7	1.7
Ga-Adangbe	Adangbe	7	1.7
Zabrama	Songhai	6	1.4
Brong	Brong	5	1.2
Wassa	Twi	5	1.2
Ada	Adangbe	4	0.9
Akim	Twi	4	0.9
Ijaw	Ijaw	4	0.9
Efutu	Efutu	3	0.7
Shai	Adangbe	2	0.5
Adja	Ewe	1	0.2
Ahanta	Fanti	1	0.2
Dagarti	Dagarti	1	0.2
Ashanti/Ewe	Ewe	1	0.2

Polyethnic interaction was accompanied by widespread multilingualism. In a survey of forty neighborhood adults, six stated they could speak only two languages; the thirty-four others reported they could speak (or in a few cases "hear" but not speak) from three to seven languages. English was widely spoken; 41 per cent of the women and 80 per cent of the men had completed at least the ten year's middle school education that Sey (1973) suggests is indicative of "stage one" competence in Educated Ghanaian English.[4] Several others with less education could also, in my experience, speak English as well as many middle school leavers. Only 9 per cent of the men, but 37 per cent of the women, had no formal education.

The facts of ethnic mixing and multilingualism support a position that the Adabraka population should be analyzed from a "many codes–one culture" point of view (see Hymes 1968) rather than from a "plural society" stance, which would direct research toward a series of at least fourteen "one language–one culture" ethnographies. Such a framework would wrench individuals out of their social contexts and tear apart households and kin groups. The relevance of the "many codes–one culture" framework may be illustrated by the multilingual situation in one household in which three ethnic identities were represented and seven languages

were used daily by the members (see Figure 1). Admittedly this was the most linguistically complex of the neighborhood households, but there were few in which at least two languages were not spoken by the members in their daily round (see Hymes 1968:31–32).

THE DOMAIN OF ETHNIC IDENTITIES IN ADABRAKA

Despite the facts that "tribe" was pointedly *not* a subject about which information was sought in the 1970 Census of Ghana and that the word "tribe" has been banned in official Ghanaian government usage since 1972, Adabraka residents in 1970–1971 and 1974 were at ease in using the word "tribe" in casual conversations, and they raised no objection to its use in interviews. As we shall see, several Adabraka informants used "tribe" in a world-wide sense including not only African ethnic identities but also such terms as "European," "American," "Lebanese," and "Chinese." "Tribe" in Ghanaian English may be glossed as "ethnic identity" in American English. We may speak then of a domain cover term "tribes" as meaningful to Adabraka people and proceed to see what is included in it.

During my fieldwork in 1970–1971 I asked fifty-eight informants: "can you tell me the tribes living in Accra today?" This group included twenty-seven women and thirty-one men, ranging in age from seventeen to seventy. In terms of their own ethnic identities, the group consisted of fourteen Kwawus, thirteen Gas, twelve Ewes, four Ashantis, three Yorubas, two Brongs, two Akwapims, two Fantis and one from each of the following identities: Shai, Nzima, Wassa, Ga-Adangbe, and Dagarti.

One Ewe housewife responded, "I don't have any interest in such things" (she was quite willing to be interviewed about other matters), but the other fifty-seven informants each provided from one to fifty-five

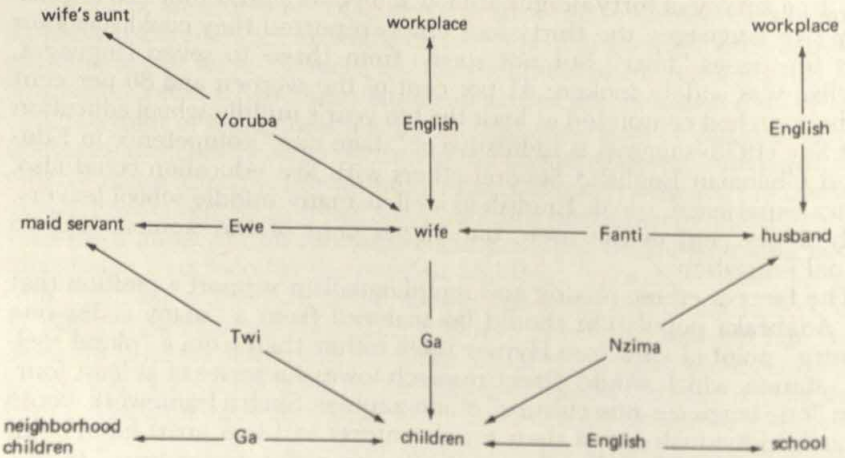

FIGURE 1. A multilingual household's daily language use.

ethnic terms. Two Kwawu market women, however, restricted their responses to a term or terms relating only to Ga, the indigenous ethnic identity of Accra. In these two cases it appears that "tribe" is not a meaningful cover term for the ethnic domain and that the question-frame was in these cases inadequate. The mean number of "tribes" listed was ten; half the informants (28/58) provided between eight and twelve terms. In all, 136 different terms were elicited.

The responses ranged from straightforward lists to "textlets" (Perchonock and Werner 1969:238). A quarter of the informants included a comment along the lines of these: "I think some of all the tribes in Ghana are in Accra," and "there are so many others; I don't know the names." The responses therefore should not be taken as either authoritative or complete from the informants' own points of view. Individuals may be uninterested (like the Ewe housewife) or very interested (like the man who offered fifty-five terms) about ethnicity, and many individuals quite willingly admit they do not know as much about the domain as there is to know.

The set of fifty-seven responses together represents an approximation of what Roberts, Chiao, and Pandey (1975:121–122) call a "larger cultural informational system" over which no single individual possesses "total informational control." While Roberts and his colleagues could not, with only one informant, "attempt to map the informational control and belief of the informed believers" in each of the two religious pantheons they investigated, I would like to present such a mapping for the much simpler domain of "tribes" in Adabraka.

In Figures 2 and 3 I have organized the set of terms using as much as possible of the information about hierarchical levels of contrast (the most salient mode of organization) as provided by the informants. I have taken the most common categories where disagreement exists, and I have added (in parentheses) hierarchical categories that help to arrange the data. The result is a view of the ethnic domain that does not correspond to what exists in the head of any Adabraka informant but that is based upon what all of them know or might come to know.[5]

THE SALIENCE OF INDIVIDUAL ETHNIC TERMS

Although the data do not provide information about the full extent of individual knowledge of the domain (for which other methods and more research time with each informant would be required), they do permit us to measure the salience of particular ethnic terms among the Adabraka population. Using Romney and D'Andrade's frequency of recall measure of salience (1964:154–155; compare Sanjek 1971:1128–1130), we may figure salience as the proportion of informants who mentioned a particular ethnic term as against the fifty-eight informants queried. As is shown in Table 2, only six terms were mentioned by at least half the informants, and only twenty-three terms by at least one-tenth of the sample. Most of the 136 terms mentioned therefore are of very low salience.

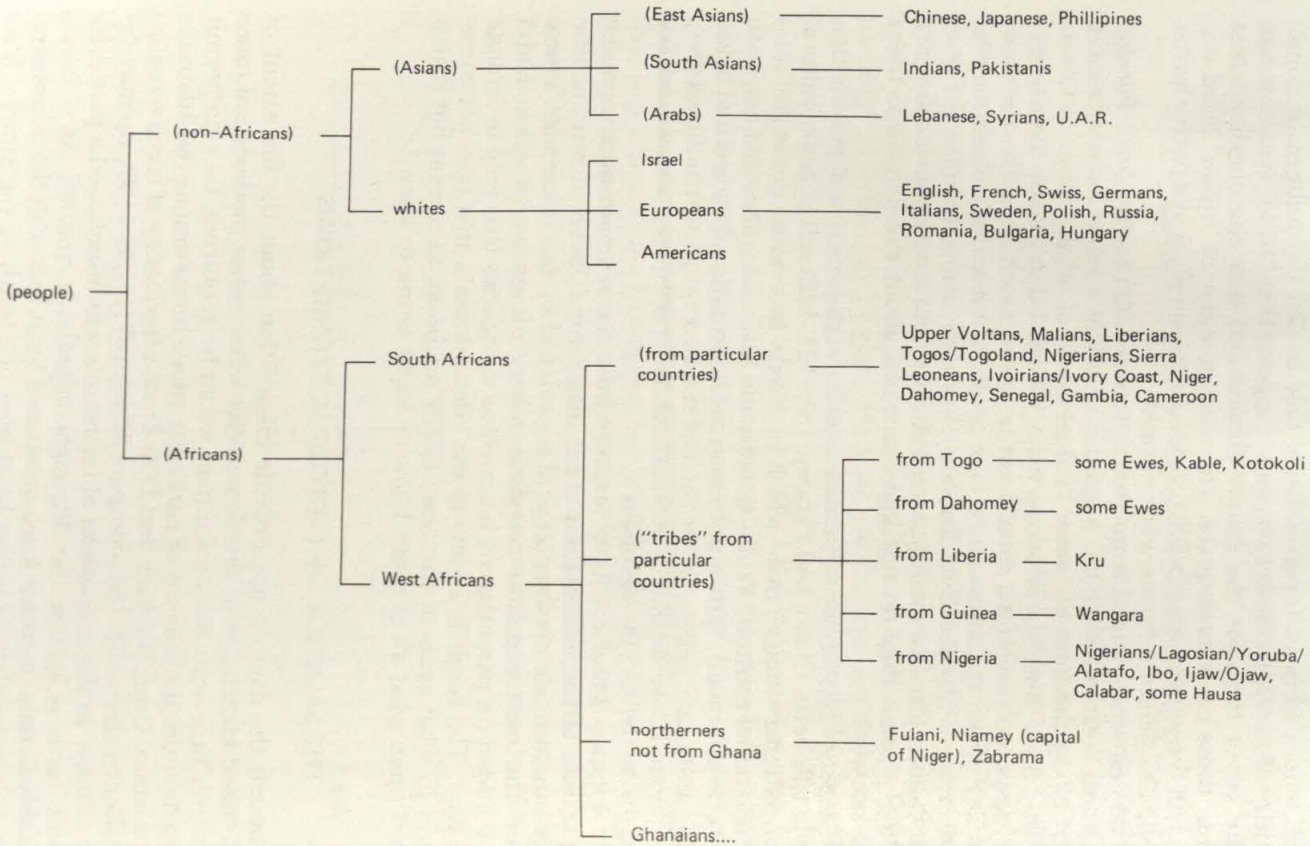

FIGURE 2. The larger cultural informational system of "tribes" in Adabraka. Where the name of a country appears, the informant response was "people from _____."

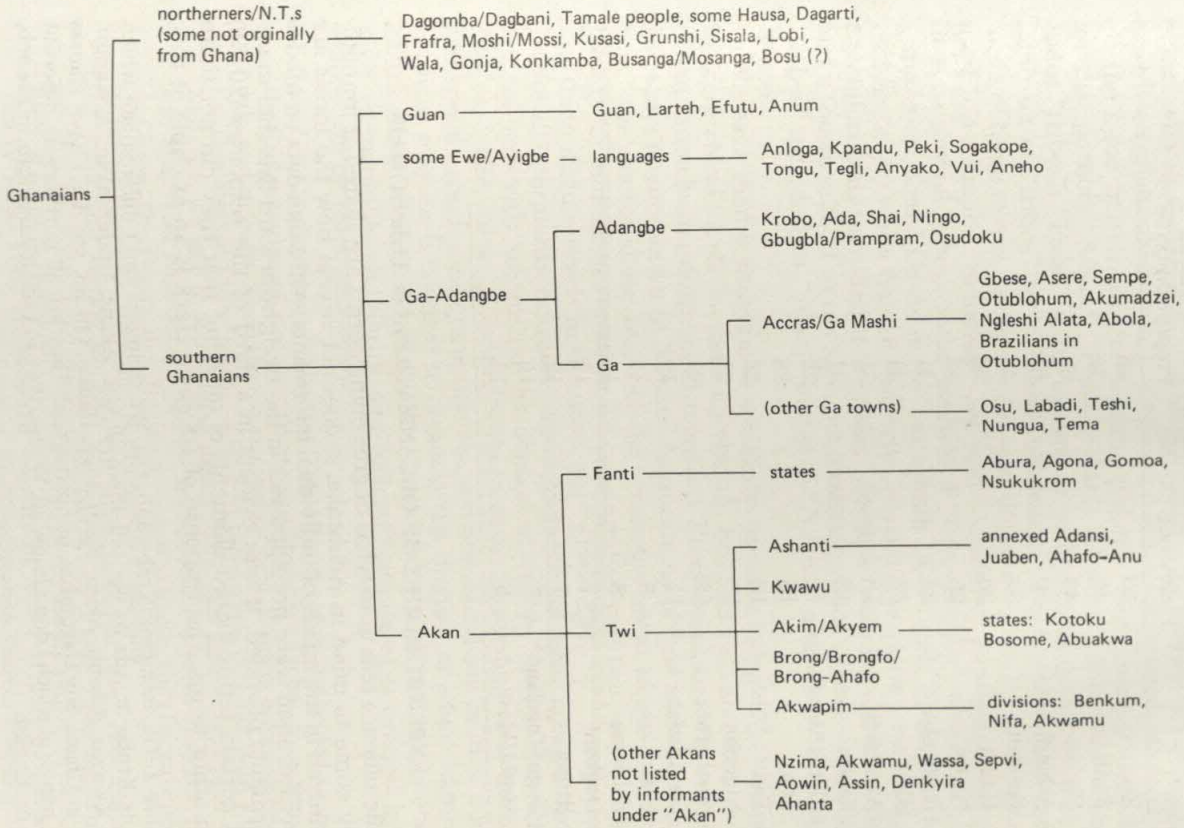

FIGURE 3. The larger cultural informational system of "tribes" in Adabraka.

TABLE 2. Salience of Twenty-three "Tribes" Mentioned by at Least 10 Per Cent of Informants

"Tribe"	Salience (N=58)	Proportion (x/58)
Ewe	50	.86
Fanti	42	.72
Ga	40	.69
Ashanti	39	.67
Kwawu	34	.59
(6) Hausa	30	.52
Nzima	25	.43
Dagomba	24	.41
Akan	18	.31
(10) Akwapim	18	.31
Krobo	18	.31
Akim/Akyem	17	.29
Ada	16	.28
Twi	13	.22
Nigerian	13	.22
northerner	12	.21
Ga-Adangbe	11	.19
Frafra	8	.14
Zabrama	8	.14
(20) Dagbani	7	.12
Brong	7	.12
Togo/Togoland	7	.12
Moshi/Mossi	6	.10

EXPLICIT MODES OF ORGANIZATION OF THE DOMAIN

While salience tells us to what degree ethnic terms are shared or immediately come to mind in Adabraka, it does not reveal how the terms are organized in the minds of individual informants. Although data on modes of organization were not apparent in the straightforward lists that many informants provided, it was evident in a third of the responses (20/58) that certain terms shared elements of meaning that they did not share with other terms. The response of a Kwawu clerk is an example of this:

> *Case 1:* Ga, including Krobo and Ada. We strangers can't differentiate which is Krobo or Ada so we call them all Gas. Akans. Under Akan, Akwapim, Kwawu, Ashanti, Akim, Fanti. Ewe. Hausas. Oh no, we don't have Hausas in Ghana. Say Dagombas, or northerners. They are all northerners, different tribes, of which I don't know all. Whites. We can't tell if American, or French, or English.

For this man the terms "Ga," "Akan," and "white" each include a number of "tribes." "Ewe" is both a "tribe" and a term that contrasts with "Ga," "Akan," and "white." "Northerner" includes several unnamed "tribes"

and is to some degree synonymous with "Dagomba" and with "Hausa," a term that is corrected to "Dagomba or northerner."

Now if this set of inclusive terms was in fact shared by the other informants who also offered hierarchical inclusive categories our task would have been relatively simple. But it was not. Not all informants who marked off the same portion of the domain used the same hierarachical term for that portion: "Akan," "Twi," "Ashanti," and (in other conversations about "tribes") "Fanti" were all used as inclusive terms to refer to the same set of "tribes" by different informants; others contrasted two or more of these terms, and some did so in different ways. Some informants used "Ga-Adangbe" to include "Ga" and "Adangbe" (of which there were in turn several types); some contrasted "Ga-Adangbe" with "Ga"; and some contrasted "Ga-Adangbe" with both "Ga" and "Krobo," a term that for others was a type of "Adangbe." In the case of the term "Nzima," two informants included it in "Akan," another excluded it from "Akan," and a fourth said, "I'm not sure; put it outside Akan." Three others listed "tribes" included in "Akan" but gave "Nzima" elsewhere in their response.

We have then a situation in which the domain is explicitly organized in hierarchical levels by some informants, but there is variability in that organization from informant to informant. Perchonock and Werner noted the same phenomenon in Navaho classifications of food and commented that "the intermediate levels of the taxonomy seem to leave the most leeway for individual variation. We have rarely found two people classifying these middle levels in the same way" (1969:234).

Not all informants, however, used a hierarchical mode of organization. Several explicitly organized the domain of "tribes" in nonhierarchical modes. I offer here as examples three cases illustrating how historical viewpoints, residential circumstances, and numerical and political considerations may each be used to organize the domain of ethnic identities. (Three more modes of organization—a second type of historical perspective, cartographic familiarity, and "official" languages as defined by the state—will be discussed below.)

Case 2 (a Ga draftsman): The Gas. The Hausas came and joined us before the Krobos and the Ewes came. We term all together Krobo—from Somanya, Ada, and all parts. Then the Akans, especially the Kwawus before the Ashantis. The latest are the Nzimas—a little in the earliest part, but with Kwame Nkrumah we got to know them plenty. The Zabramas from Upper Volta and Mali. The Nigerians, noted for trade—you see them in every village in Ghana. The Ojaws, Ibos, the last we came to know. They became popular at first as stewards, then they began to run small bars.

Case 3 (an Ewe market woman): Oh, many different ones. Take his house, we have Lagosian, Togo people, Ijaw, Twi, Kwawu, Ewe, Zabrama, so many.[6]

Case 4 (a Kwawu journalist): All the tribes in Ghana are represented. Kwawu and Ashanti. They are now the majority in Accra. Hausas and Nigerians. They were the majority before the Compliance Order. Adangbe. Fanti. Northerners, mostly in the Ashanti Region.

In view of this variability in modes of organization, I conclude that informants may use the same terms for "tribes" but place those "tribes"

within different cognitive arrangements. Two informants could each agree that a particular person is "Ewe," but they might contrast "Ewe" with other terms in different ways. As Wallace puts it, anthropologists "must recognize the possibility of a radical diversity of mazeways that have their orderly relationship guaranteed not by the sharing of uniformity, but by their capacities for mutual prediction" (1961:28).

THE IMPLICIT UNDERLYING STRUCTURE OF THE DOMAIN

I would now like to suggest that a covert level of contrast underlies the variability of both lists and modes of organization. I refer to an implicit presence in each cognitive map of the three major language groups of southern Ghana: Ga-Adangbe, Ewe, and Akan. (Guan, the other southern language grouping, consists of several geographically separated languages spoken by relatively very small numbers of people, many of whom also speak Akan languages or Ewe.)

While some informants may recognize distinctions within one of these language groups, others may not, like the Kwawu clerk (case 1) who finds it difficult to distinguish "Gas" from the "Krobo," "Ada," or other varieties of Adangbe. Speakers of Adangbe dialects find this task simple. Similarly, several Ewe informants pointed to what they called "different Ewe languages," a consideration which only one non-Ewe raised. But overriding these differences, what informant cognitive maps do have in common is the presence of one or more "tribes" from each of the three language groups.

If we first lump together those terms referring to Ga-Adangbe speakers (Ga, Accra, Ga-Adangbe, Adangbe, Krobo, Ada, Shai, Ningo), those referring to Ewe speakers (Ewe, Ayigbe, Anloga), and those terms unambiguously included in the category Akan (Fanti, Twi, Ashanti, Akyem/Akim, Akwapim, Kwawu, Brong) and then re-examine the fifty-eight responses to determine the salience of at least one term assignable to these three language groups, we find almost no variability:

Language Group	Salience	Proportion (x/58)
Ga-Adangbe	52	.90
Ewe	52	.90
Akan	54	.93

Remembering that one informant was uninterested, two others gave only Ga-related terms and adding that one woman mentioned only African countries other than Ghana, I conclude that at this implicit level of the recognition of differences in language groups, cognitive maps are almost universally congruent. I refer to this shared but implicit level as structure.[7] We may speak then of cognitive maps of the ethnic domain as being empirically variable, and as being organizationally diverse, but also as being structurally similar if not congruent. It is structure that is shared rather than organization or the bulk of empirical content.

COGNITION AND BEHAVIOR

In response to certain criticisms of the ethnoscience enterprise, several recent papers have stressed the importance of relating cognition to the behavioral performances to which particular ideological domains relate (Howe and Sherzer 1975; Johnson 1974; Keesing 1974; LeVine and Price-Williams 1974; Sankoff 1971). While fully agreeing with Kay (1970:21) that "cognition is cognition and behavior is behavior," I also believe that this necessary separation is primarily an analytic one that should be applied in collecting data, but not in the kinds of anthropological portraits we paint of the societies being studied. I would amend Johnson's statement that "the interrelation between cognition and behavior is a great and enduring issue in the social sciences" (1974:96) to read that "it is *the* great and enduring issue." Ethnography that forsakes either behavior or ideas is neither art nor science.

I obtained interview-based behavioral records of the interactions over four-day periods of twenty male and twenty female Adabraka residents. Each chronological record includes two weekdays and a weekend. They represent what Harris calls an attempt to "use the informant etically"—to have her or him "describe past and present events in conformity with the categories of significance which the ethnographer has brought to the task" (Harris 1968:576–577). (For further details and results, see Sanjek 1972, 1978.) The records provide for each actor the sequence of interactions she or he entered and the ethnic identities of each alter encountered. Analysis of the data indicates that multiactor scenes involving Adabraka residents were more likely to be polyethnic than not, but that this varies with behavioral setting, as shown in Table 3, and also from actor to actor.

What is of interest here is the ethnic identities of the alters encountered in these sample behavioral records. In all, the forty Adabraka people interacted with 1,126 alters whom they identified with sixty-nine ethnic terms. We may compare these behavioral data on which particular "tribes" were actually encountered with the cognitive material already presented. In Table 4 I have compared the twenty most salient southern Ghanaian ethnic identities with the frequency of an appearance by at least one individual of that "tribe" in each of the forty behavioral records.[8]

There is considerable correspondence between the two sets of data. While precise prediction of cognitive salience from behavioral data is not possible, a more general prediction of the first five, second five, and subsequent ten most frequently encountered southern Ghanaian ethnic identities can be made. The opposite prediction from cognitive salience to behavior is also possible.

Let me turn now to the relationship between behavior and what I have identified as the underlying structure of language group distinctions. Table 5 shows a correspondence between cognitive structure and behavior that is even closer than that between term salience and behavior. What slight differences occur in the behavioral data as between Akan, Ga-Adangbe, and Ewe may be attributable to the slightly larger number of Akan (fourteen informants) and Ga-Adangbe (thirteen informants) first language speakers than Ewe first language speakers (ten informants)

TABLE 3. Polyethnic Scenes in Forty Four-Day Behavioral Records.

Behavioral Setting	Multiactor Scenes	Percentage of Polyethnic Scenes
(a) Work and Public Places:		
Ego's workplace	273	73.6
Alter's workplace	55	63.6
Place of purchase	27	66.7
Hospital/clinic	15	73.3
Government office	4	50.0
Public transportation	9	33.3
(b) Domestic Domain:		
Ego's apartment	350	36.3
Building courtyard	136	82.4
Alter's apartment (ego's building)	25	64.0
Alter's apartment (different building)	171	39.2
(c) Leisure domain:		
Street	183	52.2
Car	45	48.9
Taxi	42	35.7
Bar	26	53.8
Restaurant/canteen	19	78.9
Church	22	31.8
Association meeting	3	66.7
Parliament session	5	60.0
Soccer stadium	1	100.0
Cinema	1	—
Total	1412	54.3

among the forty informants, as well as to the probable greater numbers of Ga-Adangbe speakers and Akans than Ewes in the population of Accra in 1970–1971.

Table 5 also lists six other language categories: *northerner*, which includes "Hausa" and any of the particular northern ethnic identities; "northerners" frequently use Hausa as a lingua franca among themselves and, in the opinion of several Adabraka residents, also speak English distinctively. *Nzima* is a southern Ghanaian language included by linguists in the Akan grouping, but one that is not intelligible to speakers of Twi or Fanti. The Nzima identity is of particular salience because it was the "tribe" of Kwame Nkrumah, and it is the fourth most numerous ethnic identity in the Adabraka neighborhood. *Nigerian* usually refers to Yoruba speakers and may be used alternatively (often in one utterance) with the terms "Yoruba," "Nago/Anago," "Lagosian," and "Alata." The term is not used to include Nigerian Hausas ("Kano people") and probably

TABLE 4. The Twenty Most Salient Southern Ghanaian "Tribes."

"Tribe"	Salience (N=58)	Cognition (Lists of "Tribes") Proportion (x/58)	Rank Order	Appearance (N=40)	Behavior (Four-day Records) Proportion (x/40)	Rank order
Ewe	50	.86	1	30	.75	2–3
Fanti	42	.72	2	21	.53	5
Ga	40	.69	3	35	.88	1
Ashanti	39	.67	4	23	.58	4
Kwawu	34	.59	5	30	.75	2–3
Nzima	25	.43	6	10	.25	8
Akwapim	18	.31	7–8	14	.35	6
Krobo	18	.31	7–8	9	.23	9–10
Akim	17	.29	9	12	.30	7
Ada	16	.28	10	9	.23	9–10
Brong	7	.12	11	3	.08	12
Shai	2	.03	12–16	4	.10	11
Wassa	2	.03	12–16	1	.03	13–18
Anum	2	.03	12–16	1	.03	13–18
Efutu	2	.03	12–16	—	—	19–20
Ningo	2	.03	12–16	—	—	19–20
Akwamu	1	.02	17–20	1	.03	13–18
Larteh	1	.02	17–20	1	.03	13–18
Sefwi	1	.02	17–20	1	.03	13–18
Ahanta	1	.02	17–20	1	.03	13–18

TABLE 5. Underlying Structure of Language Categories.

Category	Salience (N=58)	Cognition (Lists of "Tribes") Proportion (x/58)	Rank Order	Appearance (N=40)	Behavior (Four-day Records) Proportion (x/40)	Rank Order
Akan	54	.93	1	36	.90	1
Ga-Adangbe	52	.90	2–3	35	.88	2
Ewe	52	.90	2–3	30	.75	3
northerner	45	.78	4	17	.43	4
Nzima	25	.43	5	10	.25	5
Nigerian	22	.38	6	8	.20	6–7
white	6	.10	7	8	.20	6–7
Guan	5	.09	8	3	.08	8
Lebanese	4	.07	9	2	.05	9

does not include the small numbers of non-Yoruba southern Nigerians such as Ibos, Ijaws, and others. *White* includes Europeans and Americans, people who are normally spoken to in English. For some Ghanaians it may also include Chinese and Japanese people (my wife is of Japanese ancestry, and she was referred to as "white" by a number of Ghanaians). *Guan* is the fourth southern Ghanaian language group, and *Lebanese* includes "Lebanese" and "Syrian" individuals. Lebanese speak Arabic among themselves and are more likely to speak a bit of a Ghanaian language than are "whites."

These six identities or identity clusters are neither of universal structural significance nor very frequently encountered by most Adabraka residents, as Table 5 demonstrates. Their structural and behavioral importance do appear, however, to be related, and their rank order is predictable from both structural and behavioral perspectives. Were one deliberately to ask Adabraka informants about these six categories, I am certain they would be almost universally recognized. Their more limited cognitive salience in free recall, I suggest, is directly related to their low frequency of appearance in behavioral settings. Few northerners, Nzimas, Nigerians, whites, Guans, or Lebanese live in Adabraka, and most appeared in behavioral records only in workplace settings. Almost every building in the Adabraka neighborhood contains Akans, Ga-Adangbes, and Ewes.

The final comparison I may make between cognition and behavior is to compare the sixty-nine ethnic identities encountered in the behavioral records with the larger cultural informational system. About three-quarters of the terms (54/69) used to identify alters in the behavioral sequences were also offered in informant responses to the "tribes in Accra" questions. Some of those that were not (6/69) may easily be placed within the larger cultural informational system either as synonyms ("British" for "English," "Egyptian" for "people from the United Arab Republic") or as additional members of existing categories ("Adja," a "tribe" from Dahomey; "Mamprussi," a "tribe" from northern Ghana; "Navrongo," people from a town in Northern Ghana; "Volta Region, Twi-speaking" as another type of Twi).

The remaining terms (9/69), amounting to 13 per cent of the ethnic identities encountered in behavior, cannot be placed within the larger cultural informational system as described so far. These terms are "Afro-American"; "mulatto: German/Ningo"; "half-caste: American/Ewe," "Ewe/Ashanti," "Fanti/Hausa," "Ga/Kwawu," "Ga/Akim," and "Ga/Ewe." This set of terms refers to individuals who appear in the behavioral world of Adabraka residents but whose ethnic identities combine two "tribes." That none of these ethnic identities appeared in the responses to the "tribes in Accra" frame underscores the importance of studying both cognition and behavior.

"Afro-Americans" have become increasingly visible in Accra since the late 1960s. Although the term was of low cognitive salience in 1970–1971, all of the nineteen Adabraka residents I interviewed in 1974 could identify the term "Afro-American" and provide descriptive information about how they differed from Ghanaians. Exactly how "Afro-American"

will be fitted into both individual cognitive modes of organization and the larger cultural informational system if, as one may expect, the term becomes more cognitively salient may be determined only through future research.

The terms "mulatto" and "half-caste" (with the identifications of the "tribes" of the father and mother) represent another question. Individuals of African and European parentage have been part of the social fabric of Accra for centuries. The tendency has been for them, or their descendents, to blend into Ghanaian society by adopting or being given the ethnic identity of their African parent (although the example of the Ga Bannermans, an Accra family founded by a British father and an Ashanti mother in the nineteenth century, illustrates the importance of residence as well). In terms of ethnic ideology, "mulatto" and "half-caste" represent "anomalous" social types that repeatedly appear but that do not form the beginning points for persisting identities and groups.

The final set of terms, the six identifications compounding two "tribes," reflects the behavioral realities of intermarriage. According to 1960 census data, 17.9 per cent of the recorded current marriages of Ghanaian heads of households were interethnic; the figure for Accra was 23.8 per cent (Census Office 1970:238, 243). While most commonly the offspring of such marriages take only a single ethnic identity, this is not always so, as these compound labels indicate. (I may add that many more alters within the behavioral records were identified by one ethnic identity, however it was immediately added "but the mother (or father) is of 'a different' tribe.") If these compound identities represent a growing trend in identification and self-identification, we might speculate about corresponding changes in the larger cultural informational system, in cognitive salience, and perhaps also in underlying structure (where we might predict an added category of "mixed ethnic identity" evident in individual outlooks by specific compound identifications, such as "Ga/Larteh" in one case and "Fanti/Hausa" in another; this would, however, alter the basically linguistic nature of the underlying structure of ethnicity).

COGNITION AND CHANGE

There is of course no direct way to tap the cognitive outlooks of informants other than those currently living. Some limited indirect evidence might be gleaned by examining written materials. In the case of Ghana, nineteenth century histories and diaries could be combed to see what ethnic terms (and modes of organization) were then in use among literate Ghanaians. Such evidence, however, would not constitute firm ground upon which to erect models of cognitive change leading to the larger cultural informational system of Adabraka in 1970.

An alternative approach is to consider a corpus of synchronic cognitive maps as "a product of both the past and present circumstances . . . neither static nor timeless" (Howe and Sherzer 1975:443). We might then ask whether cognitive maps differing greatly from the others and terms of low salience may, almost in a Darwinian sense, be remnants of the past or harbingers of the future. From this perspective, idiosyncracy may have

evolutionary rather than eccentric import (see Pelto and Pelto 1975:14–15).

The response to the "tribes in Accra" frame obtained from a seventy year old Ga man may illustrate such a "fossil" cognitive map:

> *Case 5:* In Ga Mashi: Gbese, Asere, Otublohum, Ngleshi Alata, Sempe, Akumadze, Abola; Brazilians, in Otublohum. Kwawus, under Gbese. Nigerians. Fantis. Ashantis. Ewe people. Ivory Coast people. Sierra Leone people. Togo people.

The Gas are the indigenous ethnic group of Accra. In historical terms their centuries-old town, Ga Mashi, represents the nucleus around which the city of Accra has grown. This particular Ga informant has spent almost his entire life in Accra.[9] He grew up in a predominantly Ga community at a time when the traditional seven quarters ("Gbese, Asere . . . Abola"), each with its own chief, military company, and circumscribed geographical boundaries in Ga Mashi were of major social significance to town residents. The other "tribes" in this list include the nineteenth and twentieth century immigrant groups (Brazilians, Kwawus, Fantis, Nigerians) that then were or were becoming numerically and economically salient in Accra society.

By 1970 the Gas accounted for less than a third of the population of Accra, and the seven old Ga Mashi quarters together represented only one small neighborhood in a city more than thirty times the size of Accra in 1911. Few Adabraka residents, including even younger Gas who have grown up there, could list the seven quarters of old Accra. For most individuals it is the present behavioral importance of the particular ethnic identities they encounter and of languages they hear regularly that is encoded in cognitive outlooks.

This view of the Ga Mashi quarters as central to a consideration of "tribes in Accra," a view no other informant articulated, reflects what I hypothesize would have been far more typical of cognitive maps in the early 1900s, but only in Accra. At that time the social antecedents of the Adabraka population would have been living throughout southern Ghana, where I suggest individual cognitive maps would have reflected in a salient fashion the local social groupings corresponding to the seven quarters in Accra. As in the Ga Mashi case, these groupings within local settlements were frequently diverse in linguistic and cultural backgrounds. The important economic groups that began to appear throughout southern Ghana as the colonial era wore on—Kwawu and Nigerian traders, Ewe craftsmen and cocoa farm laborers, Ga and Fanti produce buyers and teachers from the coastal towns, Hausa and northerner butchers and manual laborers—would have come to assume cognitive salience as they became of behavioral importance in Ghanaian towns and rural areas. The movements of individuals and groups into new areas of opportunity and the corresponding increase in the volume of everyday interethnic contact (and interethnic marriage) were experienced by Ghanaians themselves as participants and observers.

What they learned and how they organized their knowledge are evident in the nature of the cognitive maps of the Adabraka population.

Cognition "reflects both the situation in which the Cuna [read Adabraka people] now find themselves and the dominant social and economic changes they have undergone in the past century, and we cannot fully understand the classification [of "tribes"] without taking these processes into account" (Howe and Sherzer 1975:443).

Finally, I would like to speculate that the synchronic cognitive materials may also provide a glimmering of things to come. A few of the informant responses to the question frame indicate the importance of education and national cultural policy as factors that may be affecting individual cognition of the ethnic domain. Changes of this sort are quite distinct from the behavioral facts of ethnic mixing and intermarriage that I have already identified as sources of past or potential changes in cognitive salience and structure. We might wish to label what I am about to discuss as cognitive change "from above" as opposed to the behavioral trajectories "on the ground."

The next case indicates that education may become a major source of change in cognitive organization. It is the response of a Fanti man who not only graduated from one of the outstanding Ghanaian secondary schools but who had training and considerable experience in surveying and civil engineering. His list of "tribes in Accra" reflects his familiarity with maps (it corresponds closely to the 1960 Census of Ghana map of the "Tribes of Ghana"), and it was the most extensive listing (fifty-five terms) of all the Adabraka informants.

> *Case 6:* Nzima; Ahanta; Aowin; Sefwi; Denkyira; Assin; Fanti, including Abura, Gomoa, Agona, Nsukukrom; Efutu; Akim, including Kotoku, Bosome, Abuakwa; Kwawu; Akwapim, including Benkum, Nifa, Akwamu divisions; Ga; Adangbe; Anum; Ewe; Ashanti, which annexed Adansi, Ahafo-Anu, Juaben; Brongfo; Gonja; Dagomba; Frafra; Grushi; Bosu; Wala. Those are from Ghana. Others, not from Ghana, are: Hausa; Mohsi; Wangara, from Guinea; Zabrama, from Mali; Kotokoli, from Togo; Ewes, from Togo and Dahomey; Yoruba; Ibo; Calabar; Ijaw; Indians; Syrians; Lebanese; Germans; Italians; Swiss. And in my office, people from Philippines, Hungary, Bulgaria, Poland.

This response approximates closely how the "authorities" on ethnicity in Ghana (the advisors to the census department for example) would answer the question. With increasing rates and lengths of education, such modes of organization may become more and more common.

But other, simpler official views of ethnicity are also available to Ghanaians "from above": the set of official languages that are taught as subjects in schools, the "tribes" appearing in versions of history in school texts, and the six African languages used by the Ghana Broadcasting Corporation (GBC)—Akan or Twi-Fanti, Ga, Ewe, Nzima, Dabgani, and Hausa (see Apronti and Denteh 1969).

The six GBC languages are of high salience to Adabraka residents, among whom radios are ubiquitous. They represent an accessible number of terms (with the other GBC language, English, the magic number seven), and they correspond closely to the underlying structure of language groupings for which I have argued.

This set of terms, used by state-sponsored institutions and referring to "languages" rather than to the now officially banned "tribes," may

well come to form a more commonly shared mode of organization for the ethnic domain in Ghana. Kwawus will still be Kwawus to themselves and their associates, but reference to a Kwawu as "Akan" or "Kwawu-Akan" (I have a case of this) may become more frequent. Ethnic identities speaking "minority languages" (Apronti and Denteh 1969) will probably be assimilated to the official language they most commonly command. (A few informants, for example, have told me that the Efutus, a Guan-speaking group centered in the town of Winneba, "are Fantis, but they have their own language too.")

The influence of the official languages as a mode of cognitive organization is apparent in this response:

> *Case 7 (a Brong woman):* Akan, Ewe, Nzima, Fanti-Akan. We have seven languages; which did I forget? Ga. Hausa.

Two other straightforward lists conformed with the official seven languages.[10]

PROPRIOSPECT AND PROSPECT: TWELVE DIMENSIONS OF VARIABILITY

I take it as firmly established that ethnographers must *assume* variability in the content of what I have called individual outlooks or cognitive maps—what Goodenough (1971) terms "propriospects" and Wallace (1961), "mazeways"; and that a number (a sample) of these outlooks must be recorded if we are to begin to understand a particular cognitive domain within a particular society. In conceptualizing variability, for both data collection and analysis, we must proceed beyond a shared/not shared contrast and attempt to identify a number of dimensions of cognitive variability. In this spirit, I would like to recapitulate the dimensions of variability that arose in my analysis of the Adabraka case.

(1) Informants vary in terms of their interest in the domain (of "tribes").
(2) Informants vary in terms of the degree to which they regard their information about the domain to be complete.
(3) The content of individual cognitive maps varies.
(4) Terms vary in salience (as measured by frequency of recall).
(5) The categories used in hierarchical modes of organization may vary.
(6) Several hierarchical and nonhierarchical modes of organization may coexist within a domain.
(7) Informants may have no explicit, conscious mode of organization; or they may formulate an ad hoc mode of organization when seeking to answer the ethnographer's question; or they may refer to a previously held explicit and conscious mode of organization.
(8) An informant may hold more than one explicit and conscious mode of organization for a particular domain; or he may admit more than one mode is valid.
(9) Underlying structures may vary in the degree to which they are shared.

(10) There may be variability between cognition (term salience, underlying structure) and experience in the behavioral realm, corresponding to the cognitive domain. In the Adabraka case, as in Johnson's analysis of planting rules and practices (1974), cognition and behavior correspond to a considerable degree. This may be otherwise.

(11) There may be variability in the degree to which terms used in, or to refer to, behavior appear (or do not, for example "Afro-American," "Fanti/Hausa") in frame-elicited cognitive outlooks.

(12) There may be variability in terms of past-oriented and what may become more common future variants as measured against central tendencies in a corpus of individual cognitive maps.

I make no claim to have identified a complete set of the dimensions of variability in cognition. I do claim, however, that each of these dimensions is researchable within any cognitive domain in any single society. In time, we may be able to make general sociocultural statements about each dimension of variability—about similarities, differences, and patterned covariation within human societies. But I suggest any search for such universals is premature. Our next step must be to uncover the empirical nature of variability along its many dimensions in particular societies. Anthropology *is* a young science, and those of us who regard ethnography as a privilege can take heart that it is.

NOTES

1. An earlier version of this paper was presented at a symposium on "The Challenge of Cognitive Variability" during the 74th annual meeting of the American Anthropological Association, December 1975, in San Francisco. I would like to thank the organizers of the symposium Jane M. Christian and Peter M. Gardner for the occasion to prepare this paper. I would also like to thank Allen Johnson for his comments and suggestions on an earlier version of this paper.
2. Fieldwork in Accra for eighteen months in 1969–1971 was supported by a Foreign Area Fellowship and a New York State Lehman Fellowship; research for two months in 1974 was funded by a Faculty Research Award from the University Committee on Research of the City University of New York. Lani Morioka Sanjek participated fully in the fieldwork, and the data presented in this paper are a product of our common work. For more information on the research and its results, consult Sanjek (1972, 1974) and Sanjek and Sanjek (1976).
3. In the 1960 Census, some 380,660 persons in the Accra Municipal Council division were classified by "tribe" according to seventy-three tribal terms and the catchall columns of "other tribes from" Ghana, Nigeria, and other African countries. Some 7,740 Europeans and Asians were also enumerated (Census Office 1964).
4. Sey distinguishes Educated Ghanaian English (E.G.E.) from both "broken English" and pidgin English, also spoken in Ghana. E.G.E. differs from "target English," British standards, primarily in terms of phonology and, to a slighter degree, lexicon; "pure grammatical errors are not very common" (1973:17).

Stage one competence in E.G.E. indicates a level at which the speaker can and does write letters in English and reads and buys the daily newspaper. In the Adabraka population, 45 per cent of the men and 16 per cent of the women had post-middle school education (teacher training, commercial, secondary, or university), which according to Sey indicates stage two or three competence in English. My fieldwork was conducted in English; as I learned of them, I came to appreciate the special qualities of Ghanaian English. I agree with those of our Ghanaian friends who regard the way English is spoken in Ghana to be "sweet-o."

5. Roberts's "larger cultural informational system" corresponds to Goodenough's "cultural pool": "the sum of the contents of all of the propriospects of all the society's members, including every system of standards of which any member happens to have knowledge. This sense of the term pertains to culture as a reservoir of resources in knowledge and skills carried by the membership of a society" (1971:42).

   The Adabraka view of "tribes" extending to include a world-wide classification of ethnic identities suggests that the Adabraka "cultural pool" is only a small eddy within a much larger "reservoir," the "larger cultural informational system" of modern world society. This "open" quality distinguishes studies of cognition in literate societies such as Adabraka from work within the "closed" cultural pools of, say, the Dani (Heider 1972) or, until recently, the Aguaruna (Berlin and Berlin 1975).

6. This response has an ad hoc air that might indicate that the informant had no readily available cognitive map. When describing other people whom they encountered, Adabraka people usually would mention occupation, wealth, education, place of residence, and kinship connections before they would mention ethnic identity; much of the time I had to ask about "tribe" in order to obtain these data systematically. There is no reason to assume that Adabraka residents have been as interested as the ethnographer in figuring all the ins and outs of ethnic identity in the urban setting. The other social characteristics I have just listed are at least as important as ethnicity for purposes of describing and classifying an ego's alters. For many individuals they are more important than "tribe." For some urban Ghanaians there may be no mode of organization for the ethnic domain in a conscious, explicit sense. Like the Ewe woman in this case, they may be simply aware that there are "many different" ethnic identities in Accra. When asked to list "tribes," their responses were truly ad hoc and should not be taken to reflect a well-formed mode of organization. I would argue that there is a range of variation with respect to both the degree to which individuals weight ethnicity in describing and classifying others and the degree to which informants formulate and draw upon conscious, explicit modes of organization of the ethnic domain.

7. The model of language categories does not follow in strict inductive fashion from the data. It was rather an intuitive "hunch," which I decided to test out with the data. I would agree with Horton (1967:166) that the scientific enterprise indeed involves the formulation of such "models of underlying reality" as a necessary complement to the processes of induction and deduction. I would also agree with Caws (1969) that intuitive "leaps" (compare Horton 1967:166) that are empirically verified constitute a regular and recurrent feature of scientific discovery.

   In Horton's terms, the empirically different pairs, "Ashanti, Krobo," and "Kwawu, Ga" may be represented as AB and XY. "Only in relation to some model of underlying reality . . . can we come to see that A and X, B and Y, so different in the eye of the casual observer, are actually outward manifes-

tations of the same kinds of events. . . . And only thus can we come to see AB, XY as two instances of a single underlying process or regularity" (1967:166).

In choosing the word "structure" I do not claim to share the understanding of the concept as used by structuralist anthropologists in the tradition of Lévi-Strauss, although much of what Maranda (1972:330–331) writes about the concept of "structure" does seem to fit what I have in mind. To use an old analogy, I would compare "structure" with the (relatively) invariable number and organization of bones comprising the skeleton; "empirical" outlooks vary as do the colors and textures of skin and hair that the skeletal structure supports.

8. Thirty-eight of the informants from whom I obtained behavioral records also provided lists of the "tribes in Accra today." While comparison between each list and each record and between the two bodies of data for this group of thirty-eight is possible, it would be burdensome and would sacrifice the remaining material. I shall confine my analysis to a comparison between the cognitive data from the fifty-eight informants and the behavioral data from the forty. Both samples, though not random, are similar in their ethnic mix and are drawn from the same neighborhood population. They diverge from each other in the following ways:

	Kwawu	Ga	Ewe	others
40 behavioral records	.25	.25	.25	.25
58 cognitive outlooks	.22	.24	.21	.33
11 building population	.25	.18	.16	.42

9. He differs in this regard from another Ga man of the same age who had been posted as a civil servant throughout southern Ghana during his working career. This man's list of ethnic identities had none of the Ga Mashi parochialism; it conformed to the central tendencies in the corpus of outlooks.
10. The term "Dagbani," the language of the Dagomba people and one of the GBC broadcast languages, appeared in seven "tribes in Accra" responses. Five of these individuals also mentioned "Dagomba" although none made the ethnic group/language distinction. One Yoruba man however told me, "they are mostly alike. I don't know the difference." I suspect no alter encountered in behavior would call himself "Dagbani"; the salience of this term in Adabraka is no doubt due to its use in "official" contexts "from above." The (low salience) usage of the term "Brong-Ahafo" as a "tribe" and perhaps some of the more common occurrence of "Ga-Adangbe" and "Akan" may also reflect "official" influence now socially established.

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16

Growth and Development of Folk Botanical Life-Forms in the Mayan Language Family

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Folk botanical life-form terms designate the most inclusive, comprehensive classes of plants regularly found in folk botanical taxonomies. An earlier survey of 105 globally distributed languages showed that five life-forms, "tree," "grerb" (small herbaceous plant), "vine," "bush," and "grass," are added to languages in a highly regular manner (Brown 1977a). Figure 1 presents the lexical encoding sequence for these life-form classes.

This paper[1] presents evidence showing that botanical life-form growth in languages of the Mayan family of Mesoamerica has followed the encoding sequence of Figure 1. The life-form inventories of daughter languages of Proto-Mayan contain no combinations of life-form terms indicating that encoding paths other than those in Figure 1 have been followed. Protolanguage reconstruction also provides evidence consistent with the encoding sequence. Reconstructed systems have precisely the kind of botanical life-form inventories to be expected if the proposed encoding sequence were in fact predictive of life-form growth. Reconstruction also shows that life-form growth occurred relatively late in Mayan language history: the vast majority of life-form terms have been added in the last thousand years or so.

This study also treats systematic ways in which Mayan languages have acquired folk botanical life-forms, continuing the investigation of principles of botanical nomenclatural development begun by Brent Berlin (1972).

FOLK BOTANICAL LIFE-FORM ENCODING SEQUENCE

The critical features associated with the five universal botanical life-form taxa are as follows:

"tree"	large plant (relative to the plant inventory of a particular environment) whose parts are chiefly ligneous (woody)
"grerb"	small plant (relative to the plant inventory of a particular environment) whose parts are chiefly herbaceous (green, leafy, nonwoody)
"bush"	plant of intermediate size (relative to "tree" and "grerb")
"grass"	herbaceous plant with narrow, often bladelike or spear-shaped leaves
"vine"	plant exhibiting a creeping or twining or climbing stem habit

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† Source: *American Ethnologist* 6:366–385, 1979.

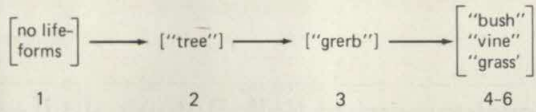

FIGURE 1. Lexical encoding sequence for folk botanical life-forms (revised from Brown 1977a).

The lexical encoding sequence of Figure 1 is interpreted as a series of stages in the growth of folk botanical life-form vocabularies. Stage 1 languages have no botanical life-forms. At Stage 2 "tree" is lexically encoded. "Tree" in early-stage languages is often considerably broader in actual plant membership than "tree" in later stage languages. It frequently includes bushes and shrubs, and sometimes even ligneous vines, in addition to trees.

At Stage 3 a second life-form, "grerb," is lexically encoded. "Grerb" in early stage languages characteristically encompasses most, if not all, plants not included in "tree." The encoding of "tree" and "grerb" is attributed in part to the general human tendency to classify by means of binary opposition. "Tree" and "grerb" terms encode the contrast "large plant"/"small plant." (For detailed explanations of life-form encoding regularities see Brown 1977a, 1977b, 1979; Witkowski and Brown 1978; and Brown and Witkowski 1980.)

From Stage 4 to Stage 6, "bush," "vine," and "grass" are lexically encoded. These additional categories encompass plants previously included in "grerb" and/or "tree." The lexical encoding of "bush," for example, involves pulling bushes and shrubs from the range of "grerb" or "tree," or from the ranges of both, as the case may be. Consequently, as life-forms are added from Stage 4 to Stage 6, the ranges of "grerb" and "tree" tend to shrink.

MAYAN FOLK BOTANICAL LIFE-FORMS

Mayan languages are spoken in a relatively continuous zone encompassing parts of Guatemala, Belize, Honduras, southern Mexico, and the Yucatan Peninsula. The family also has an outlier, Huastec, spoken in northern Veracruz and eastern San Luis Potosí, Mexico. Figure 2 locates all Mayan languages, with the exception of Huastec, in geographic space.

A genetic classification of Mayan languages is presented in Table 1 (adapted from Brown and Witkowski 1979). Time depths associated with major and minor groupings are also given in Table 1 as minimum number of centuries. Chronological estimations follow Kaufman (1976:103).

Table 2 lists folk botanical life-form terms for twenty-six of thirty-one Mayan languages (data from Acatec, Sacapultec, Sipacapa, Uspantec, and Teco were not available). Language sources, listed in the Appendix, are of three general types: (1) published studies of folk botanical systems, (2) personal communications from language scholars with firsthand field knowledge, and (3) dictionaries and other word lists. Sources of types 1 and 2 are more reliable than those of type 3 with respect to thoroughness

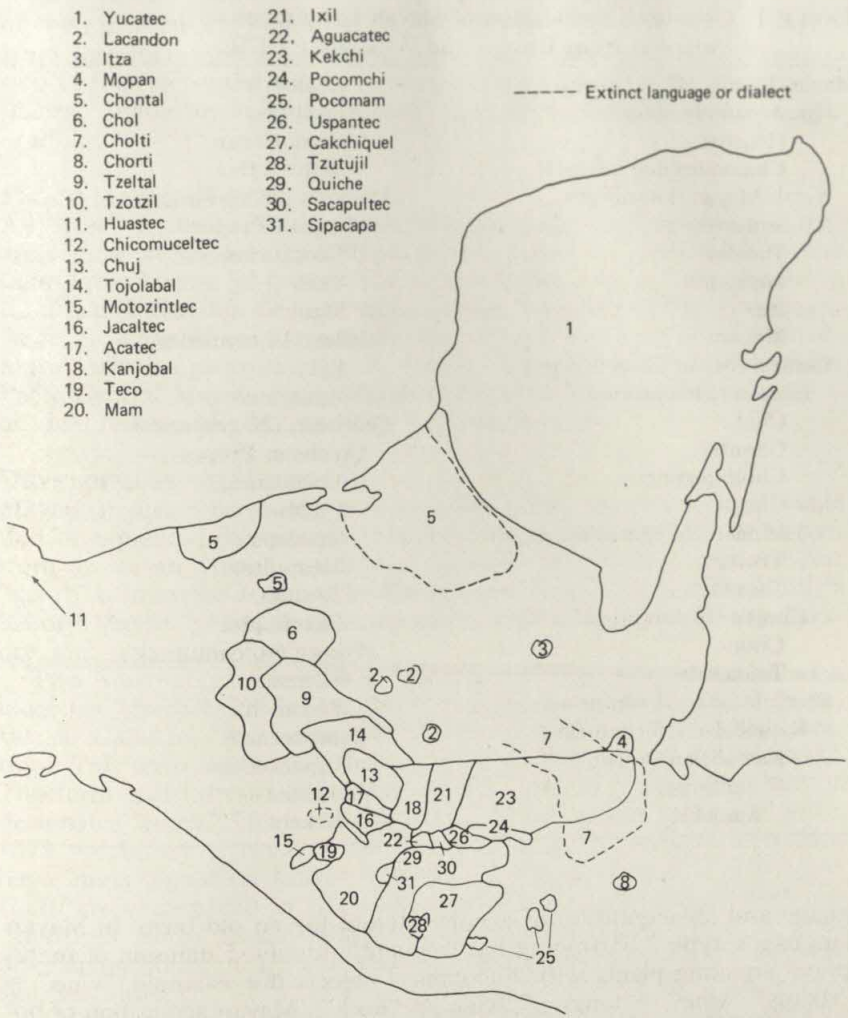

FIGURE 2. Modern distribution of Mayan languages in Mesoamerica (adapted from Fox 1977).

and accuracy of reference. Source type is indicated for each language case in Table 2.

BOTANICAL LIFE-FORM ACQUISITION

Mayan languages have acquired folk botanical life-forms through both innovation and borrowing. All cases of borrowing, save one, have entailed diffusion within the language family. Two types of borrowing have occurred: (1) acquisition of a term previously unknown to the accepting lan-

TABLE 1. Genetic Classification of Mayan Languages (Adapted from Brown and Witkowski 1979).

Mayan Family (42 centuries)	Acatec
Huastecan (9 centuries)	Jacaltec
Huastec	Motozintlecan
Chicomuceltec (extinct)	Motozintlec
North Mayan (Yucatecan)	Mamean (26 centuries)
(10 centuries)	Mamean Proper
Yucatec	(15 centuries)
Lacandon	Teco
Itza	Mam
Mopan	Ixilan (14 centuries)
Central Mayan (30 centuries)	Ixil
Cholan (14 centuries)	Aguacatec
Chol	Quichean (26 centuries)
Chontal	Quichean Proper
Cholti (extinct)	(10 centuries)
Chorti	Quiche
Tzeltalan (14 centuries)	Sipacapa
Tzeltal	Sacapultec
Tzotzil	Tzutujil
Chujan (16 centuries)	Cakchiquel
Chuj	Pocom (10 centuries)
Tojolabal	Pocomam
South Mayan (34 centuries)	Pocomchi
Kanjobalan (15 centuries)	Uspantecan
Kanjobalan Proper	Uspantec
(7 centuries)	Kekchian
Kanjobal	Kekchi

guage and (2) acquisition of a new referent for an old term. In Mayan languages, type 2 borrowing has frequently involved diffusion of metaphors equating plants with nobotanical objects (for example, "vine" = "string," "vine" = "tongue," "vine" = "neck"). Mayan acquisition of botanical life-forms through type 2 borrowing has been limited, for the most part, to "vine."

Mayan languages have innovated botanical life-forms by developing new terms and by adapting old ones. Development of new terms has involved overt marking. Overt marking of "grerb," for instance, entails creation of a compound label for the life-form consisting of a term for "tree" and a modifier. These labels literally translate "short tree," "small tree," "tree-opposite," and so on (see Brown 1977a:334–335 for a discussion of overt marking in the innovation of life-forms).

Converting existing terms into life-form labels has involved two mechanisms of lexical change: (a) use of metaphor and (b) expansion of reference. As mentioned above, metaphors equating plants and nonbotanical objects often underlie development of "vine" in Mayan languages. Expansion

of reference entails increasing the designative range of a term. Mayan languages, for example, have developed "grerb" life-forms through expansion of the referential ranges of words designating "weeds," "medicinal plants," "underbrush," and "greens" to small herbaceous plants in general.

Proto-Mayan "Tree"

All "tree" terms in Table 2 demonstrate sound correspondences that are regular in Mayan. These terms are all cognates descended from the same Proto-Mayan (pM) stem. The reconstructed stem is * ṯe·ʔ, following Kaufman (1969:168). Because all modern reflexes of *pM* * ṯe·ʔ designate "tree," it is logical to conclude that this form designated "tree" in Proto-Mayan and that protostages in the language family's history (for example, Proto-Central Mayan, Proto-Mamean, Proto-Kanjobalan Proper, and so on) had reflexes of *pM* * ṯe·ʔ that referred to "tree."

Development of "Grerb"

Mayan languages rarely share "grerb" labels that are cognate (see Table 2). Consequently, these terms cannot be traced to a single Proto-Mayan stem as Mayan "tree" words can. All lines of evidence indicate that "grerb" terms were acquired by Mayan languages very late in the family's history. Mayan "grerb" term acquisition entailed borrowing, overt marking, and expansion of reference.

Two North Mayan languages, Yucatec and Lacandon, share *šiw* as a label for "grerb." *Šiw* is also found in a third North Mayan language, Itza, as a label for "medicinal herb" (Pierre Ventur, personal communication). This term was clearly borrowed from Nahuatl (see Whorf 1948). The form *šiwitl* occurs in several modern Nahuatl dialects, in which it designates "grerb." The word also is included as a "grerb" label in Molina's 1571 vocabulary of classical Nahuatl. While no other Mayan languages have forms cognate with North Mayan *šiw*, forms cognate with Nahuatl *šiwitl* are widespread in Uto-Aztecan languages (see Whorf 1948:6 for examples). North Mayan languages probably borrowed the term after developing from Proto-North Mayan, which broke up some ten centuries ago (see Table 1).

"Grerb" is overtly marked in at least five Mayan languages: Mopan, Itza, Kanjobal, Aguacatec, and Jacaltec. The Mopan and Itza "grerb" term, *pokčeʔ*, has the constituent, *čeʔ* "tree" and a modifier, *pok-*. The modifier is a frozen element that has no independent meaning today (Rosemary Ulrich, personal communication). The word *pokčeʔ* may have labeled "grerb" in Proto-North Mayan (pNM). Reflexes of pNM **pokčeʔ* are found in Yucatec and Lacandon with somewhat different, but clearly allied, referents. Yucatec *pokčeʔ* refers to "low brush," and Lacandon *pakčeʔ* labels "weed" (pNM *o* becomes Lacandon *a* in certain contexts). Referents of the Yucatec and Lacandon forms may have developed through reduction of reference. This restrictive process often occurs when two lexical items having similar, if not the same, meanings come into confrontation (Brown 1978). The referential ranges of Yucatec and Lacandon reflexes of a pNM **pokčeʔ* (designating "grerb") could have shrunk

TABLE 2. Mayan Folk Botanical Life-Form Lexicons.

Languages	Source[a] type	"tree"	"grerb"	"vine"	"grass"	"bush"
Huastecan						
Huastec (Potosino)	3	*teʔ*	*cʼoho·l*	*punaθ*	*tom*	*ʔᵇ*
Huastec (Veracruzano)	1	*teʔ*	*čʼoho·l*	*čʼah*	*tom*	*wayelteʔ*
Chicomuceltec	3	*teʔ*	*poš*	*čʼah*	*sin/sim*	?
Yucatecan						
Yucatec	1	*čeʔ*	*šiw*	*à·kʼ*	*súʔuk*	(no term)
Lacandon	2	*čeʔ*	*šiw/robʼir*	*a·kʼ*	*suʔuk*	*kabʼar u čeʔer*
Itza	2	*čeʔ*	*pokčeʔ*	*akʼ*	*suʔuk*	(no term)
Mopan	2	*čeʔ*	*pokčeʔ*	*akʼ*	*suʔuk*	(no term)
Cholan						
Chol	2	*t̃eʔ*	*pimel*	*akʼ/əkʼ*	*ham*	*sah t̃eʔ*
Chontal	2	*teʔ*	*bʼənəla/bʼələna*	*akʼ*	(no term)	(no term)
Cholti	3	*teʔ*	*sakul*	*sak čʼahan*	?	?
Chorti	1	*teʔ*	*kʼopot*	*čʼa·n*	*ak*	*pʼen*
Tzeltalan						
Tzeltal (Tenejapa)	1	*teʔ*	*wamal*	*akʼ*	*ak*	(no term)
Tzeltal (Bachajon)	3	*teʔ*	*haʔmal*	*čʼoš/ išiʔmʔakʼ*	*ak*	?
Tzotzil (Chamula)	2	*teʔ*	*vomol*	*akʼ*	*yašaltik*	*voc voc teʔ*
Tzotzil (Zinacantán)	2	*teʔ*	*cʼiʔlel*	*akʼ*	*hobʼel*	(no term)
Tzotzil (San Andrés)	2	*teʔ*	*cʼiʔlel*	*akʼ*	*hobʼel*	(no term)

Chujan						
Tojolabel	2	*teʔ*	*akᶜ*	*akʼ*	*ak*	?
Chuj	1	*teʔ*	*ʔaŋ*	*čʼaŋ*	(no term)	(no term)
Kanjobalan						
Jacaltec	2	*teʔ*	*telax*	*aqʼ*	(no term)	(no term)
Kanjobal	3	*teʔ*	*mokʼan (teʔ)**	*čʼan*	*akʼun*	?
Motozintlec	3	*teʔ*	*wiʔkuy**	*kʼahan*	*suan*	?
Mamean						
Mam	2	*ceˑʔ*	*kʼul*	*aqʼ*	*qʼeʔn*	(no term)
Aguacatec	3	*ceˑʔ*	*wiʔ(ceʔ)**	*aqʼ*	*kʼim*	?
Ixil	2	*ceʔ*	*kʼoač*	*aqʼ/qul*	(no term)	(no term)
Quichean						
Kekchi	2	*čeʔ*	*pim/qʼeˑn*	*kʼaham/ quˑl*	(no term)	(no term)
Pocomchi	2	*čeˑʔ*	*kʼičeˑʔ* (also includes "vine")	*kʼaham/qul*	(no term)	(no term)
Pocomam	2	*čieʔ*	*čaˑm*	*kʼahaˑm*	*kʼim*	*akʼun čieʔ* (rare)
Tzutujil	2	*čeˑʔ*	*iˑčaˑx*	*raš kʼaˑm*	*kʼim*	(no term)
Cakchiquel	1	*čeˑʔ*	*qʼayis/qʼos*	*(reš) kʼaʼn*	(no term)	?
Quiche	2	*čeˑʔ*	*qʼayes**	(no term)	(no term)	(no term)

[a] *Source Type:* 1—published study; 2—personal communication; 3—dictionary.
[b] *Symbols:* * = may not be full-fledged "grerb" term; ( ) surrounds optional element; ? = unknown if language has term; / = occurs between alternative terms.
[c] Furbee-Losee (personal communication) identifies *ak* only provisionally as a "grerb" label. This term more specifically refers to "grass" in Tojolabal.

to "low brush" and "weed," respectively, upon confrontation with the intrusive "grerb" term *šiw* (borrowed from Nahuatl).[2]

The principal constituent of the Kanjobal "grerb" label, *mok'an teʔ*, is *teʔ*, which designates "tree." The addendum *mok'an* translates "shortened" or "short" (*mok'an* is traced to pM **muk'* "to squat," Brown and Witkowski 1979). Thus, "shortened tree" or "short tree" is the literal meaning of the Kanjobal term for "grerb."

Aguacatec "grerb" translates literally "head of tree": *wiʔ* "head, top, tip" + *ceʔ* "tree." This label is metaphorical in nature, involving the equating of small, green, herbaceous plants with the green, herbaceous parts of trees, that is, leaves and ramifying branches that are situated at the top of trees.

The Jacaltec "grerb" label, *telax*, is derived from *teʔ* "tree" and the "locative of abundance" suffix *-lax*. The term is now a frozen form (Christopher Day, personal communication). According to Day (1977, and personal communication), *telax* is a double-sensed word designating (1) land with certain types of plant cover, or under cultivation, and (2) plants other than large trees that are not cultivated. In addition, *telax* refers to trees and shrubs *if they are small*, a usage also found in other Mayan languages for "grerb" (Brown 1977a:333).

"Grerb" life-forms in thirteen Mayan languages have developed through expansion of reference. This has involved elevation of terms to life-form status originally designating small plant categories not traditionally treated as formal classes in folk botanical taxonomy, such as "weed," "medicinal plant," "underbrush" or "thick vegetation," and "greens."

Most, if not all, Mayan languages have terms for useless, bothersome plants, but few languages share labels for "weed." "Weed" terms in Mayan languages are often traced derivationally to forms that originally meant "bad," "rotten," "spoiled," "dirty," and so on. The Lacandon "grerb" term *rob'ir*, for example, developed from a "weed" word derivationally linked to a form meaning "badness." The Lacandon label is cognate with *lob'il* "badness" found in Yucatec and Cholti (Proto-Mayan **l* becomes Lacandon *r* in certain environments). Roys (1931:211) and Barrera Marin, Barrera Vazquez, and Lopez Franco (1976:33) cite the stem form *lob'* as a Yucatec label for "weed." Lacandon *rob'ir* similarly developed the referent "weed" from "badness." Once this botanical usage became established, the category was extended to small herbaceous plants in general, including both useful and useless species.

Cakchiquel *q'ayis* and Quiche *q'ayes* designate "grerb." Related forms in other Quichean languages focus referentially on useless objects, especially useless plants: Tzutujil *q'ayis* "weeds, garbage" and Pocomam *q'ahes* "brush, bushes, scrub, rubbish, junk." Proto-Quichean (pQ) **q'ahis*, from which the Cakchiquel, Quiche, Tzutujil, and Pocomam forms are descended (*pQ *h* becomes *y* in certain Quichean languages), is derived from Proto-Mayan **q'ah* "rotten." Thus, the original botanical referent of pQ **q'ahis* appears to have been "rotten plant" or "weed." The Cakchiquel and Quiche "grerb" terms then are traced developmentally to expansion of a "weed" category. The fact that reflexes of pQ **q'ahis* designate "grerb" in both languages is most likely accountable to diffusion.

Zinacantán and San Andrés Tzotzil "grerb," *c'iʔlel*, developed from a term designating "garbage" in general and "weeds" in particular. A related word, *c'·lel*, denotes "garbage" in neighboring Tojolabal. These terms are derived from Proto-Mayan **c'i·l* "dirty, rotten, spoiled" (compare with Kaufman 1969:168).

The Mamean Proper languages, Mam and Teco, demonstrate the related forms *k'ul* "grerb" and *k'u·l* "weed," respectively. Related terms are found in Tojolabal, *k'ul* "weeds, brush, and brushland," and in Chuj, *k'ul-tak* "brushland." This distribution is probably a result of recent innovation and diffusion. Whatever the exact historical details, Teco, Chuj, and Tojolabal indicate that "grerb" developed as a referent of Mam *k'ul* through extension of a "useless plant" term to small herbaceous plants in general.

The Potosino and Veracruzano Huastec "grerb" term, realized respectively as *č'oho·l* and *č'oho·l*, acquired its contemporary application through reference expansion. In eighteenth-century Huastec this term meant "craggy and brambled" (Tapia Zenteno 1767:68). Thus, some two hundred years ago the word was principally associated with "useless vegetation."

Several Mayan languages derived "grerb" from terms for "medicine." While parts of certain trees occasionally are used as medicines by Mayan speakers, most of their remedies are "herbal." Thus development of Mayan "grerb" terms from words for "medicine" involved extending the range of a term for small medicinal plants to small plants in general.

Chicomuceltec, a now extinct Huastecan language, employed *poš* as "grerb." Related forms are limited to the two Tzeltalan languages. The stem *poš* designates medicines in general in both Tenejapa Tzeltal and San Andrés Tzotzil. In Bachajon Tzeltal it refers to a specific type of remedy, that is, ointments. Kaufman (1972:114) reconstructs **poš* as the Proto-Tzeltalan term for "medicine." It is likely that Chicomuceltec borrowed *poš* from the Tzeltalan languages because its close genetic relative, Huastec, does not have this form. In addition, Chicomuceltec speakers and speakers of Tzeltal and Tzotzil are known to have been in contact through A.D. 1700 (Lawrence H. Feldman, personal communication). *Poš* almost certainly was borrowed into Chicomuceltec with the meaning "medicine" or "medicinal plant" and subsequently developed its "grerb" application through expansion of reference.

The Chuj "grerb" term, *ʔaŋ*, originally designated "medicinal plant" and thus developed through reference expansion. The term's earlier meaning appears today only in a compound form, *ʔaŋ tun* "medicine." A related form, *ʔan*, found in Tojolabal designates "medicine." Cognates also occur in neighboring Kanjobalan languages: Jacaltec *ʔaŋ* "medicine" and Kanjobal *ʔan* "liquor." Because these terms are limited to Chujan and Kanjobalan languages, it is unlikely that they are traceable to a Proto-Mayan stem. Instead, this form probably originated in one of these two subgroups and diffused to the other. The original meaning was clearly "medicine."

One of Kekchi's two "grerb" words, *q'e·n* (see Table 2), probably developed from a term for "medicinal plant." The related Mam *q'eʔn* designates

both "liquor" and "grass." Use of *q'eʔn* as a label for "liquor" indicates that the word probably denoted "medicine" in the past. Mayan languages often encode "liquor" by use of established terms for "medicine," no doubt as a consequence of the special importance attached to *aguardiente* (Spanish for "cane alcohol") as a curative agent. If *q'eʔn* meant "medicine," the contemporary referent "grass" reflects an earlier connection of the Mam word with grasses of medicinal value. Such reasoning leads to the tentative conclusion that both Mam *q'eʔn* and Kekchi *q'e·n* are derived from an earlier form that designated "medicinal plant."

"Grerb" terms in Tenejapa Tzeltal and Chamula Tzotzil, *wamal* and *vomol* respectively, are derived from Proto-Tzeltalan (pT) **wamal.* A reflex of the latter, *vomol,* found in San Andrés Tzotzil, designates "medicinal plant." Proto-Tzeltalan **wamal,* then, probably labeled "medicinal plant." If so, the "grerb" applications of reflexes of pT **wamal* developed through expansion of reference. Sharing of this usage by Tenejapa Tzeltal and Chamula Tzotzil is probably due to diffusion.

"Grerb" terms in Kekchi, Chol, and Pocomam derive from Proto-Mayan forms referring to "thickness" and "depth." Reflexes of these forms first developed the botanical referent "thick/deep/heavy vegetation," that is, "underbrush." Terms for "underbrush" were then elevated to life-form status through expansion of reference. This involved extending the ranges of words denoting closely packed small plants to small plants in general.

Kekchi and Chol "grerb" terms are *pim* and *pimel* respectively. A related form, *pimil,* is used by speakers of Zinacantán Tzotzil to designate "heavy undergrowth." All three items are linked to the Proto-Mayan word for "thick," **pi·m* (compare Kaufman 1969:171). Kekchi *pim* and Chol *pimel* had referents similar to that of Zinacantán Tzotzil *pimil* before developing as "grerb" labels.

Pocomam *ča·m* "grerb" referred to "heavy vegetation" before achieving life-form status. A related form, Pocomchi *čam,* means "thick, bushy." Similarly, *čamok* is used by speakers of Kekchi to indicate "getting grown over." These Quichean forms are traced to Proto-Mayan * *ṭ am* "deep, thick, heavy" (Proto-Mayan * *ṭ* becomes Quichean *č*).

Tzutujil developed "grerb" by expanding the referential application of a term for "greens." The category "greens" includes edible leafy plants such as cabbage, lettuce, and turnips. A word for "greens," **i· ṭ a·x,* reconstructs for Proto-Mayan. Tzutujil is the only Mayan language to develop a reflex of this term, that is, *i·ča·x* (pM * *ṭ* becomes Tzutujil *č*), as a label for small leafy plants in general.

Tzutujil *i·ča·x* is polysemous, designating both "grerb" and "greens" (Jim Butler, personal communication; Andrade 1946a). Because "greens" are types of "small herbaceous plants," Tzutujil *i·ča·x* labels two different botanical classes, one of which is immediately included in the other. This is reminiscent of examples cited by Berlin (1972:65–69) in which a culturally significant type of tree and a "tree" life-form are labeled by the same term. This is the case, for example, in many Southwestern U.S. Indian languages, in which words for "cottonwood tree" are also applied to trees in general (Trager 1939). Berlin (1972) points to such polysemous

uses of arboreal terms as indicative of the manner in which "tree" life-forms sometimes arise, that is, through expansion of reference of a term designating a specific kind of tree (such as the cottonwood).

Reference expansion involving arboreal classes as described by Berlin (1972) does not differ in essential details from that underlying derivation of Mayan "grerb" categories. In both instances a label associated with some botanical class becomes associated with a more inclusive class, and the latter encompasses the former. In the case of "tree," a term for a certain kind of tree develops as a label for trees in general. In the case of "grerb," a term for a certain class of small herbaceous plants develops as a label for small herbaceous plants in general.

Expansion of reference is achieved by relaxing definitional constraints pertaining to category membership. This can be illustrated through use of a simple feature model. For example, two semantic features, *+large ligneous plant* and *+widespreading branches and a loose open habit,* are plausibly associated definitionally with a term (in any particular language) for "cottonwood tree." Expansion of reference of such a term to trees in general occurs when the latter feature is relaxed and the feature *+large ligneous plant* alone is associated with it. Similarly, sets of two semantic features are plausibly associated with terms for "weed," "medicinal plant," "underbrush," and "greens." All such terms share the feature *+small herbaceous plant* and differ with respect to a second feature. The "weed" term is also associated with the feature *+useless;* the "medicinal plant" term with *+curative;* the "underbrush" term with *+closely packed in growth;* and the "greens" term with *+edible.* "Grerb" develops as an expanded meaning of these terms when the latter features are relaxed and the feature *+small herbaceous plant* alone is pertinent.

Development of Mayan "grerb" terms through expansion of reference has involved precisely the categories listed in the preceding paragraph. "Grerb" life-forms in Lancandon *(rob'ir),* Cakchiquel, Quiche, Zinacantán and San Andrés Tzotzil, Mam, and Huastec are derivationally linked to "weed"; those in Chicomuceltec, Chuj, Kekchi *(q'e·n),* Tenejapa Tzeltal, and Chamula Tzotzil to "medicinal plant"; those in Kekchi *(pim),* Chol, and Pocomam to "underbrush"; and that of Tzutujil to "greens" (see Table 2).

Words associated with plant categories of a general nature, such as "weed," "medicinal plant," "greens," are natural candidates for elevation to "grerb" status because their ranges of reference already extend to many different kinds of small herbaceous plants. Future research will almost certainly show that terms for similar botanical classes have developed as "grerb" labels through expansion of reference in other language families. Preliminary investigation by the author of the development of "grerb" in Indo-European languages indicates the pervasiveness of these patterns.

Expansion of Reference and "Grerb" Term Polysemy

Expansion of reference can lead to loss of the prior meaning of a word. After expansion has occurred, the original, less comprehensive application of a term may be discarded. This need not happen immediately, however.

Indeed, it is likely that expansion of reference will have the immediate effect of creating a polysemous term.

Berlin (1972) cites several examples of botanical polysemy in which a "tree" life-form and a "tree" generic are labeled by the same word. Polysemy of this sort characterizes some Mayan "grerb" labels. "Grerb" and "greens," for example, are distinct referents of Tzutujil *i·ča·x*. Several other Mayan "grerb" terms derived through expansion of reference have also retained their original narrower applications.

The Zinacantán Tzotzil "grerb" term, which developed through expansion of "weed," is polysemous. Laughlin (1975:102) glosses *c'iʔlel* as "grerb" and "weed" respectively. The narrower application of the term is reflected in compounds: *c'iʔleltik* "expanse of weeds" and *sc'iʔlelinoh* "weed infested." Zinacantán *c'iʔlel*, then, is associated with two distinct plant classes, one of which ("weed") is immediately included in the other ("grerb").

Other examples include "grerb" terms in Quiche, Potosino Huastec, Kekchi, and Chamula and San Andrés Tzotzil. Edmonson (1965:98) glosses the Quiche "grerb" term, *q'ayes*, as "hay, corn straw, grass, weeds, scrub, garbage." Retention of the nonbotanical referent "garbage" indicates that the term is still associated referentially with useless things in general and with useless plants ("weeds") in particular (see also Teletor 1959). In Potosino Huastec both "grerb" and "weed" have been maintained as distinct referents of *c'oho·l* (Larson 1955). (On the other hand, Veracruzano Huastec *č'oho·l* designates only "grerb," Brown 1971, 1972.) The Kekchi "grerb" term, *pim*, has retained "underbrush" as a referent (Freund 1976:128), and the Chamula Tzotzil "grerb" term, *vomol*, has maintained its original referent, "medicinal plant" (Garcia de León 1971).

Forms related to Bachajon Tzeltal and Chorti "grerb" terms, *haʔmal* and *k'opot* respectively, are not found in other Mayan languages. Thus, evidence external to these languages is lacking that might illuminate derivational histories of these words for "grerb." Internal evidence, however, in the form of polysemy indicates that these histories have involved similar details. Both *haʔmal* and *k'opot* designate small herbaceous plants in general and "wild vegetation" in particular. In addition, both are used as adjectives meaning "uncultivated" or "wild." Development of "grerb" in Bachajon Tzeltal and Chorti has probably entailed extending the ranges of terms originally restricted referentially to small uncultivated plants.

Development of "Vine"

Words for "vine" are often polysemous, also designating such things as snakes, worms, tongues, necks, belts, whips, cords, ropes, and strings. In Mayan languages, such polysemy is indicative of the way in which "vine" terms have developed. Most Mayan words for "vine" originally denoted nonbotanical objects, either natural or man-made, characterized by length and varying degrees of flexibility.

A number of Mayan languages share "vine" terms (see Table 2). Such sharing is due to borrowing rather than genetic relationship. Moreover, this borrowing has not always involved diffusion of lexical items. In many instances languages have simply borrowed the idea that a term for some

long, flexible, nonbotanical objects, which they already share with a lending language, can also be used as a "vine" label.

Innovation of "vine" by Mayan languages has involved use of metaphor. For example, Tzutujil *raš k'a·m*, literally "green string," designates "vine." Expressions translating "green string" label "vine" in two other Quichean languages: Uspantec *rašk'aʔm* (Campbell 1977:50) and Cakchiquel *reš k'a·n*. Sometimes the stem *k'a·n* "string" minus the modifier *reš* is used by speakers of Cakchiquel to denote "vine." Deletion of the modifier constitutes an example of *ellipsis*, which, in conjunction with use of metaphor, often underlies lexical change (Brown 1978). If Cakchiquel drops "green" altogether from the expression "green string," the unitary label for "string" will acquire the additional referent "vine." Cognate forms in three other Quichean languages, Kekchi *k'aham*, Pocomchi *k'aham*, and Pocomam *k'aha·m*, are polysemous, designating "string" and "vine." The "vine" referent of these items probably developed through ellipsis, that is, through deletion of the modifier "green" from an expression translating literally "green string."

Quiche lacks a full-fledged "vine" term but, nevertheless, has a word for "string," *k'aʔm*, cognate with those found in other Quichean languages (Edmonson 1965, and personal communication; Wick and González 1975). This indicates that the Quichean metaphor for "vine" developed after the breakup of Proto-Quichean Proper about ten centuries ago. The lateness of "vine" is also suggested by the fact that the metaphor "green string" remains lexically intact in some contemporary Quichean languages. Such metaphors tend to become truncated through ellipsis rather quickly. Widespread diffusion, then, accounts for the fact that several languages of the Quichean subgroup have related terms for "vine."

Words for "string" in Quichean languages are derived from pM **k'ax-aŋ/am*. Reflexes of the latter are found in virtually all Mayan languages. In nearly all cases these designate materials used for tying, for example, string, thread, rope, cord, twine.

In addition to the Quichean cases, seven other Mayan languages employ reflexes of pM **k'ax-aŋ/am* as labels for "vine" (see Cholti, Chorti, Veracruzano Huastec, Chicomuceltec, Chuj, Kanjobal, and Motozintlec in Table 2). This distribution for the most part is the result of borrowing within Mayan. Because of the contiguous geographical distribution of Mayan languages (see Figure 2), the family shows many examples of internal diffusion (Campbell 1977:115). A diffused feature is apparent if it crosscuts language subgroup boundaries. The latter condition pertains to reflexes of pM **k'ax-aŋ/am*, that designate "vine" in the seven languages mentioned above. Two major subgroups, Central Mayan and South Mayan, and three minor subgroups, Cholan, Chujan, and Kanjobalan, are crosscut (see Figure 2).

Two geographic language clusters (other than Quichean) designate "vine" with reflexes of pM **k'ax-aŋ/am:* (1) Chorti and Cholti and (2) Motozintlec, Chicomuceltec, Chuj, and Kanjobal (see Figure 2). The idea of using a word for "tying material" as label for "vine" (or as a constituent of a label for "vine") may have diffused to both clusters from Quichean languages. It is, of course, possible that the idea was invented in one or

both clusters. Whatever the historical details, at least one language, Kanjobal, is readily identifiable as a certain borrower, Kanjobal borrowed more than the metaphor "tying material" = "vine." It in fact borrowed its label for the life form that is, *ç'an*, which refers to "vine" alone. The Kanjobal reflex of pM **k'ax-aŋ/am* should have an initial *k'* rather than *ç*. This phonological irregularity identifies Kanjobal *c'an* as a certain loan.

"Vine" words in Chicomuceltec and Veracruzano Huastec are derived from a Proto-Huastecan (pH) reflex of pM **k'ax-aŋ/am*, that is, pH **č'a·h*. Although both dialects of Huastec, Potosino and Veracruzano, use reflexes of pH **č'a·h* to denote "rope," respectively *c'a·h* and *č'ah*, only Veracruzano Huastec applies its reflex to "vine" as well. It is likely that Veracruzano Huastec developed *č'ah* as a label for the life-form quite recently. This conclusion is supported by the fact that Andrade (1946b) lists *č'ah* as an entry for "rope" and an entirely different word, *hut'Ruk*, as an entry for "vine" in a Veracruzano vocabulary compiled in the 1930s. Thus, in the 1930s either Veracruzano Huastec *č'ah* had yet to be used as "vine," or its use as such had yet to become widely established. Brown (1972) recorded *č'ah* as the Veracruzano "vine" term in 1969. These lines of evidence indicate that pH **č'a·h* did not mean "vine" in addition to "rope." Consequently, "vine" must have been independently acquired by Chicomuceltec and Veracruzano Huastec sometime after the breakup of Proto-Huastecan some nine centuries ago. This is a reasonable assumption because the "rope" = "vine" metaphor is global in distribution and, thus, has been independently invented many times.

A number of Mayan "vine" terms are derived from the Proto-Mayan word for "tongue," **a·q'* (compare Fox 1977:227–228, 233–234). Reflexes of pM **a·q'* designating "tongue" are found in all Mayan languages except Chicomuceltec and Huastec. Reflexes of pM **a·q'* denoting "vine" are limited to thirteen geographically contiguous languages: Yucatec, Lacandon, Itza, Mopan, Chol, Chontal, Tzeltal, Tzotzil, Tojolabal, Jacaltec, Ixil, Aguacatec, and Mam (see Table 2 and Figure 2). "Vine" reflexes of **a·q'* crosscut languages of major subgroups (Central and South Mayan) and minor subgroups (Cholan, Chujan, and Kanjobalan), strongly suggesting that the metaphorical equation "tongue" = "vine" diffused widely.

The metaphor "tying material" = "vine" is analogically more compelling than "tongue" = "vine." The human tongue and tongues of many other mammals are short and thick, hardly reminiscent of vines. On the other hand, tongues of various reptiles and amphibians are long and thin, having more in common with plants possessing a creeping or climbing stem habit than, say, the human tongue. In view of this it is interesting that reflexes of pM **a·q'* "tongue" in some Mayan languages have acquired as referents certain nonanatomical objects other than vines that are characteristically long, thin, and flexible. For example, in Pocomam and Pocomchi *a·q'* designates "snake," and *-aq'il* in Ixil labels "root." In addition, a number of languages that use **a·q'* reflexes as "vine" labels also apply the latter to belts, whips, ropes, and so forth: Yucatec *ak'* "belt," Itza *ak'* "whip," San Andrés Tzotzil *ak'il* "strap," Tojolabal *ak'* "lasso, rope."

There is evidence that at least one Mayan language, rather than bor-

rowing the metaphor "vine" = "tongue," borrowed an actual term for "vine" traced to pM *$a \cdot q'$ "tongue." Tzotzil uses two phonologically distinct reflexes of *$a \cdot q'$ as labels for "tongue" and "vine," *ok'* and *ak'* respectively. Tzotzil *ok'* "tongue" is a regular phonological development from pM *$a \cdot q'$. Tzotzil *ak'* "vine" is a loan that came after development of *ok'* "tongue" from the Proto-Mayan stem. Neighboring languages, such as Tojolabal and Tzeltal, are likely lending agents.

Three geographically adjacent languages, Ixil, Kekchi, and Pocomchi, use *qul*, or a variant thereof, to refer to "vine." The proximity of these languages and the fact that distribution of this usage crosscuts languages of two South Mayan subgroups, Quichean and Mamean, suggests diffusion. Like other Mayan "vine" terms, *qul* developed "vine" as a referent through use of a metaphor equating botanical and nonbotanical objects. The stem *qul* designates "neck" in many South Mayan languages, including all Mamean languages except Teco and all Quichean languages except Pocomam, Pocomchi, and Kekchi. Pocomchi and Kekchi *qul* probably labeled "neck" in the past, losing the latter usage after acquiring "vine" as a referent. Ixil is the only South Mayan language that uses *qul* to denote both "neck" and "vine."

All three languages using *qul* as a "vine" label have alternative terms for the life-form (see Table 2). The Ixil words *aq'* and *qul* actually distinguish two general types of vines. The stem *aq'* designates nonwoody vines with flexible stems that can be used as tying material. Ixil *qul*, on the other hand, denotes thick, ligneous vines that do not easily bend (Glenn Ayres, personal communication). This distinction reflects differences between anatomical parts to which these terms were initially applied: long, thin, and flexible tongues and long, thick, and somewhat rigid necks.

Finally, in addition to string, rope, tongue, and neck, one other long and pliant nonbotanical object probably constituted the original referent of a Mayan "vine" label: Bachajon Tzeltal *č'oš* and Chol *č'oš* designate "vine" and "worm" respectively.

Development of "Grass"

"Grass" life-forms in most Mayan languages have developed through expansion of reference. In each case expanded categories were initially restricted to "roofing grass" or "thatch." The derivational history of Mayan "grass" labels is reflected in polysemy. "Grass" terms in Zinacantán and San Andrés Tzotzil, Chol, Veracruzano Huastec, Aguacatec, Pocomam, and Tzutujil (see Table 2), for example, have retained their original referent, "thatch."

Some Mayan "thatch" words designated "flat, open grassland" before developing as labels for roofing grass. Chontal *ham* "field" (Smailus 1975), for example, is cognate with Chol *ham*, a polysemous term denoting "grass" and "thatch." (Both Cholan items are almost certainly related to words in Tzeltalan and Chujan languages whose meanings center in one way or another on the concept "openness": Bachajon Tzeltal *xamal* "open, clear," Tzotzil *hamal* "wide, open, clear," Tojolabal *-ham* "to

open.") Similarly, Chol *hob'el* "flatland" and Tojolabal *hob'* "plain" are cognate with Zinacantán and San Andrés Tzotzil *hob'el* "grass, thatch." As a third example, Chicomuceltec and Veracruzano Huastec reflexes of Proto-Huastecan **tom* denote "plain (grassfield)" and "grass, thatch" respectively.

Reflexes of Proto-South Mayan **k'im* "thatch" occur in most South Mayan languages. These have developed "grass" as a referent in a Mamean (Aguacatec) and in two Quichean (Pocomam and Tzutujil) languages. Conceivably one of these subgroups influenced the other with respect to elevation of **k'im* reflexes to life-form status. However, because derivation of "grass" terms from words for "thatch" constitutes a pattern in Mayan (and in other language families as well), it is possible that all three languages independently developed "grass" through expansion of reference.

Three Mayan languages, Chorti, Tzeltal, and Tojolabal, use *ak* as a label for "grass." The word also means "thatch" in Yucatec and Chuj and was polysemous in Cholti, designating both "thatch" and "grassland." Speakers of eighteenth-century Tzotzil apparently used the term in reference to "field" (*akiltik* [*aquiltic*, original orthography], Garcia de León 1971:38). Diffusion accounts for this item's distribution because it crosscuts major and minor subgroup boundaries (Central Mayan, North Mayan, and Cholan). The term diffused either with the referent "grassland" or with the referent "thatch" or, conceivably, as a polysemous word designating both. Either Tojolabal or Tzeltal, which are geographically adjacent languages belonging to different subgroups, elevated *ak* to life-form status and then lent it to its neighbor.[3] Development of the term as a Chorti "grass" label most likely occurred independently.

Development of "Bush"

"Bush" life-forms occur in only six Mayan languages (see Table 2). All but one of these, Chorti, have overtly marked labels for the life-form. "Tree" terms constitute modified elements of these labels. Literal translations are as follows: Lacandon *kab'ar u čeʔer* "low tree," Chol *sah tẽʔ* "little tree," Chamula Tzotzil *voc voc teʔ* "tree in round clumps," Veracruzano Huastec *wayelteʔ* "sleeping tree," and Pocomam *ak'un čieʔ* "young tree."

GROWTH OF FOLK BOTANICAL LIFE-FORMS IN MAYAN

Mayan folk botanical life-form inventories range in size from two to five terms (see Table 2). No Mayan language has a combination of life-form terms indicating that encoding paths other than those outlined in Figure 1 have been followed. For example, the combination "grerb," "grass," and "bush" (without "tree" and "vine" terms) could occur, but this and other out-of-sequence combinations are not found in Mayan languages. Evidence of this sort from a large sample of globally distributed languages is the basis for asserting the existence of a universal lexical encoding sequence for folk botanical life-forms (Brown 1977a:323–324).

Language reconstruction also provides evidence consistent with the life-form encoding sequence. Only "tree" is cognate in all Mayan languages, indicating that "tree" alone reconstructs as a botanical life-form for Proto-Mayan (* *ṭe·ʔ*). On the other hand, if, for example, "grerb" reconstructed but not "tree," this would contravene the predicted order in which languages acquire botanical life-forms. Mayan languages, then, have precisely the kind of reconstructed life-form system one would expect if the lexical encoding sequence were in fact predictive of life-form growth.

All lines of evidence indicate that Proto-Mayan and other protostages in Mayan language history (for example, Proto-Central Mayan, Proto-Mamean, Proto-Kanjobalan Proper) were at Stage 2 in life-form growth (see Figure 1), having only one item, "tree." The only possible exception is Proto-North Mayan, which may have had a "grerb" term (**pokčeʔ*) and a "grass" term (**suʔuk*).[4] "Grerb," "vine," "grass," and "bush" life-forms of contemporary Mayan languages developed after the breakup of protolanguages, in some cases diffusing widely within the family.

That only one "grerb" term possibly reconstructs for a Mayan protolanguage merely reflects the fact that very few modern Mayan languages share labels for "grerb." Indeed, in some instances dialects of a single language designate "grerb" by use of different terms (see Table 2). This indicates that words for "grerb" developed very late in Mayan language history despite the fact that "grerb" is universally one of the first two life-forms to be lexically encoded (see Figure 1). This conclusion is further supported by the fact that the derivational histories of most Mayan "grerb" terms are recoverable and, hence, have not been obscured by linguistic changes that tend to accumulate over extended periods of time.

If, as reconstruction suggests, "tree" or "large plant" pertained to Proto-Mayan and other Mayan protolanguages, "grerb" or "small plant" probably existed for speakers of these languages as an *implicit* contrastive class lacking an established label but potentially identifiable through productive language use. In other words, speakers probably referred to "grerb" through the spontaneous innovation of polynomial or compound labels such as "nontree," "short tree," "small tree," "tree-opposite," and so on. Overt marking of "grerb" in several Mayan languages (Mopan, Itza, Kanjobal, Aguacatec, and Jacaltec) is evidence that such productive expressions were employed in the past by Mayan speakers in reference to small herbaceous plants. Established "grerb" terms showing overt marking were situationally produced labels that managed to gain currency and became stable.[5]

My interpretation of the growth of folk botanical life-forms in the Mayan family is summarized as follows. Proto-Mayan, spoken about four thousand years ago, possessed only one botanical life-form term, "tree." This state continued in daughter languages for about three thousand years. Approximately one thousand years ago Mayan botanical life-form lexicons began to increase in size, so that today all Mayan languages have "tree" and "grerb" and most possess life-forms beyond these. Some life-form concepts, especially "vine," diffused widely during the latter period.

SOCIETAL COMPLEXITY AND LIFE-FORM GROWTH

Most botanical life-form growth has occurred very late in Mayan language history, the vast majority of life-form terms having been added to lexicons in the last thousand years or so. Preliminary study of the growth of life-forms in Indo-European suggests that these languages as well have encoded most of their botanical life-forms quite recently (that is, in the last two thousand years or so), and brief and unsystematic surveys of other language families (for example, Uto-Aztecan, Athapaskan, and Austronesian) suggest the same for them. It appears that botanical life-form growth generally has taken place during a relatively recent period of human history. This phenomenon may relate to the fact that size of folk botanical life-form lexicons and societal complexity are highly positively correlated (Brown 1977a:328–332). Thus, as societies have moved toward high levels of political integration, social stratification, and technological development, growth of life-form lexicons has accelerated. Because the former has occurred for humanity in general during the last four thousand years or so, the major phase of life-form growth has occurred at approximately the same time.

The positive correlation of societal complexity and life-form growth is traced to the increasing separation of man from direct reliance and dependence on the natural world as societies become more complex. The typical individual in a small-scale society can commonly name and identify four to eight hundred separate plant species (Berlin, Breedlove, and Raven 1974; Conklin 1954; Hays 1976), whereas the typical member of modern urban society might manage to name and identify only forty to eighty such species. Several observers have noted that folk systems of plant and animal nomenclature tend to decay from the bottom up or, in other words, that terms for specific and generic classes are lost as societies become more complex (Berlin 1972; Brown 1977a, 1979; Dougherty 1978). As specific and generic terms disappear from folk systems, general terms, such as life-form names, become increasingly useful, so useful in fact that they tend to increase in number. The relatively recent global growth of botanical life-form lexicons, then, indexes a relatively recent overall decrease of interest in and interaction with the plant world.

CONCLUSION

In this paper folk botanical life-form lexicons for various stages in Mayan language history have been reconstructed. These show that Mayan life-form growth has conformed to the encoding sequence: no life-forms ⟶ "tree" ⟶ "grerb" ⟶ "bush," "vine," "grass." This study has also focused on ways in which Mayan languages have acquired life-form categories with the aim of elucidating principles underlying nomenclatural development. Some specific observations concerning life-form acquisition are:

1. Many botanical life-forms have been acquired by Mayan languages through borrowing. Two types of borrowing occur; (a) that involving an actual life-form term previously unknown to an accepting language

and (b) that involving acquisition of a new referent for a term already present in a language through diffusion of metaphors. Mayan acquisition of life-forms through type b borrowing has been limited, for the most part, to "vine."

2. Innovation of "grerb" life-forms by Mayan languages has occasionally involved overt marking. Overt marking of "grerb" entails creation of a compound label for the life-form consisting of a term for "tree" and a modifier.

3. Innovation of "vine" life-forms by Mayan languages has involved use of metaphor. Mayan "vine" terms originally denoted nonbotanical materials, either natural or man made, characterized by length and varying degrees of flexibility. Metaphorical equations underlie use of Mayan words for the latter as labels for "vine." These include "string" = "vine," "rope" = "vine," "tongue" = "vine," "neck" = "vine," and, possibly, "worm" = "vine."

4. Innovation of "grass" and "grerb" life-forms by Mayan languages has frequently involved expansion of reference. Expansion of reference occurs when a word labeling a certain class develops as a label for a more inclusive class, the latter encompassing the former. In Mayan languages, words for "roofing grass" or "thatch" have become "grass" labels, and words for "weed," "medicinal plant," "underbrush," and "greens" have become "grerb" labels through expansion of reference. These developments do not differ in principle from those described by Berlin (1972), involving derivation of "tree" life-forms from generic "tree" categories.

NOTES

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2. This confrontation apparently worked out differently in Itza, where *šiw* rather than *pokčeʔ* shrank in reference to "medicinal herb."

3. The term *ak* apparently has acquired a "grerb" application in Tojolabal through expansion of reference (see Table 2).

4. The "grass" term *suʔuk* almost certainly diffused to daughter languages of Proto-North Mayan.

5. Papago (Mathiot 1964), a Uto-Aztecan language, is a modern example of a Stage 2 language whose speakers recognize "grerb" implicitly. Mathiot

(1964:158) describes for Papago speakers a "subclass" of plants having no "taxonomic tag," encompassing wild perennials. Plants of this unlabeled group are "defined negatively as not belonging with either cacti or trees and bushes."

APPENDIX

Sources for Mayan languages listed in Table 2 are as follows: *Aguacatec* Andrade (1946c), McArthur (1976); *Cakchiquel* Andrade (1946a), Campbell (1971), Pantaleon de Guzman (1704), Rosales et al. (1976); *Chicomuceltec* Sapper (1912), Termer (1930); *Chol* Attinasi (1973, and personal communication), Wilbur Aulie (personal communication), Schumann G. (1973), Viola Warkentin (personal communication), Whittaker and Warkentin (1965); *Cholti* Morán (n.d.); *Chontal* Kathryn Keller (personal communication); *Chorti* Wisdom (1949); *Chuj* Andrade (1946c), Breedlove and Hopkins (1970, 1971a, 1971b), Hopkins (1967), Judie Maxwell (personal communication), McQuown (1976a); *Huastec (Potosino)* Larsen (1955); *Huastec (Veracruzano)* Brown (1971, 1972); *Itza* Schumann G. (1971), Pierre Ventur (personal communication); *Ixil* Glenn Ayres (personal communication); *Jacaltec* Christopher Day (1977, and personal communication); *Kanjobal* Andrade (1946c), McQuown (1976b); *Kekchi* Ray Freeze (personal communication), Freund (1976); *Lacandon* Phillip Baer (personal communication), William Fisher (1973, and personal communication), Ray Freeze (personal communication); *Mam* Andrade (1946c), Richard Reimer (personal communication); *Mopan* Rosemary Ulrich (personal communication), Ulrich and Ulrich (1976); *Motozintlec* Sapper (1912); *Pocomam* Thom Smith-Stark (personal communication); *Pocomchi* Linda Kay Brown (personal communication), Mayers (1958); *Quiche* Munro Edmonson (1965, personal communication), Wick and González (1975); *Tojolabal* Furbee (1974), Louanna Furbee-Losee (personal communication), Mendenhall and Supple (1949); *Tzeltal (Tenejapa)* Berlin, Breedlove, and Raven (1974); *Tzeltal (Bachajon)* Slocum and Gerdel (1971); *Tzotzil (Chamula)* Ken Jacobs (personal communication); *Tzotzil (Zinacantán)* Robert M. Laughlin (1975, and personal communication); *Tzotzil (San Andrés)* Mrs. A. Delgaty (personal communication), Delgaty (1973), Ken Weathers (personal communication); *Tzutujil* Jim Butler (personal communication); *Yucatec* Barrera Marin, Barrera Vazquez, and Lopez Franco (1976), Roys (1931), Swadesh, Álvarez, and Bastarrachea (1970).

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V

Context and Behavior

To this point, the articles in this book have dealt primarily with cultural schemata of one type: classification systems. The issues we have focused on—universality and relativity, semantic extension and transfer, and variability and change—have been considered in connection with classification systems of plants, animals, colors, body parts, material objects, kinsmen, ethnic identities, and personality traits. Covert conceptual categories in classification systems have received some attention, but for the most part the cultural constructs with which we have thus far been concerned are overtly encoded in lexical items. One of the aims of this section will be to present recent studies of cultural schemata other than classification systems, many of which are not represented in lexical items and are generally difficult, if not impossible, to verbalize.

Another aim of this section will be to consider research that not only describes cognitive structures and processes, but also indicates how these aspects of cultural competence are actually used in generating and interpreting sociocultural performance. It will be remembered from the discussion of these concepts in Section I that this distinction is between a cognitive system of knowledge, beliefs, and values and the use to which this mental equipment is put in orienting, discussing, categorizing, defining, negotiating, and understanding actual behavior in the physical world. As Keesing argues in Chapter 2, cultural competence and sociocultural performance are intricately interconnected and a full understanding of either culture or behavior requires a detailed examination of the interconnectedness of the two.

A third aim in this section will be to examine the role of context in the interrelationship between culture and behavior. As will be shown, sociocultural performance generated and interpreted on the basis of cultural competence is generated and interpreted relative to features of the social situations in which it occurs. Contexts and behaviors exert reciprocal influences on each other: culturally interpreted contexts are crucial in determining the behaviors that can be appropriately generated, and culturally generated behaviors reciprocally influence the appropriate interpretation of contexts. Because context so thoroughly influences the intricate interconnectedness of cultural competence and sociocultural performance, it must be thoroughly examined if this interconnectedness is to be understood.

THE INTERPRETATION OF WHAT IS HAPPENING

In Chapter 17, Charles O. Frake discusses methodological approaches in cognitive anthropology, and reflects on how they relate to general conceptions of the interrelationships among language, culture, cognition, and behavior. After reviewing the accomplishments and shortcomings of widely-used approaches, he argues that cognitive anthropologists should not be concerned with devising more elaborate or refined verbal elicitation instruments, but rather with understanding what society members are actually engaged in doing when they talk. According to Frake, the main interest of cognitive anthropologists should be in how society members propose, defend, and negotiate interpretations of what is happening. He argues that, if they are to understand their informants' cultures, they must learn to observe and interpret behavior and experience in the same way that their informants do.

One of the obstacles that ethnographers must overcome in acquiring this cultural knowledge is that informants often cannot verbalize the knowledge they use in making sense of life. As Frake points out, for example, informants cannot explain the sound categories of their language to a linguist because these very real cognitive categories exist out of their conscious awareness. Sound categories can only be determined indirectly from the cues that informants focus on in formulating their interpretations of sounds. Frake's point is that, in learning to interpret "what is going on now" in the terms of their informants, ethnographers must also adopt this approach because informants often cannot explicitly describe the features of appearance, sound, expression, body stance, and movement that they use in formulating their interpretations.

Contextual Frames

Frake suggests that social contexts provide the basic means of making sense of behavior and experience. Interpretations, he argues, are formulated on the basis of contextual frames, which are cultural constructs used by society members to organize their understanding of what is happening. Contexts, from this perspective, are not out there in the world to be seen by anyone who cares to look; they are cultural resources that are used by society members and should be used by ethnographers to formulate interpretations of behavior and experience.

In bringing contextual frames to the attention of cognitive anthropologists, Frake draws on the work of such prominant social theorists as Gregory Bateson (1972) and Erving Goffman (1974). A classic example from Bateson can be recounted to illustrate the notion of contextual frame. In considering the kinds of communication that occur in sequences of interaction, Bateson describes the behavior of two monkeys he observed while visiting a zoo. The monkeys were engaged in an interaction sequence comprised of individual behaviors similar to the behaviors that occur in monkey combat; however, the sequence was not combat, it was play. Bateson suggests that the interaction was framed as play because the metacommunicative message "This is play" was conveyed on the basis of certain subtle cues of bodily posture, facial expression, and the like. This contextual frame, then, provided for the interpretation that what was going on, although it very closely resembled serious fighting, was really just nonserious playing. As Bateson so felicitously puts it, "the playful nip denotes the bite, but it does not denote what the bite denotes" (1972, p. 180). Behaviors

that look like combat but are not are contextually framed as denoting only combat behaviors and not what combat behaviors themselves denote. That they are similar to and may come dangerously close to being combat behaviors is what makes play so intrinsically facinating and exciting not only for monkeys, but for humans as well.

The "slip-fights," or verbal duels, that Frake discusses in his chapter are contextually framed in much the same way. As he points out, it is apparent in George Carlin's comedy routine about slip-fights among white adolescent males in an urban Philadelphia neighborhood that remarks about mothers (or muh-thuhs) may be framed as real or ritual insults—the former are serious insults that occur as part of or that result in real fighting, whereas the latter are nonserious or playful insults that occur in verbal dueling games. And it is the fact that ritual insults so closely resemble real insults that gives them their interest for participants. Frake additionally notes, however, that, for the audience of Carlin's comedy routine, the remarks about mothers were not framed or interpreted as either real or ritual insults in a fight or game; for his audience the remarks were humorous anecdotes recounted as part of a "show." For the audience of Frake's oral presentation of his chapter, the remarks about mothers were not framed as insults in a fight or a game or anecdotes in a show, but as illustrations given to make a point in a scholarly "talk." The point here is that a particular behavior may be subjected to multiple reframings, and that each new contextual frame provides for a different interpretation.

Social Events

As will be seen in his chapter, Frake is interested in contextual frames for the light they shed on the nature of social occasions or events. Recently, as Frake indicates, significant progress has been made in determining what makes a certain sequence of behaviors a certain social event. Frake identifies a number of the dimensions that distinguish events such as shows, talks, and classes in middle-class American society (for example, the formality dimension and the risk dimension) and argues that these dimensions of cultural structure provide the basis for ethnographic descriptions that are not organized in terms of arbitrary and irrelevant schemes for dividing cultures into their component parts (for example, religion, economics, and politics), but instead rely on the cultural distinctions people use to organize their world. It is not, then, an impossible expectation that in the future cognitive anthropologists will produce ethnographic descriptions of whole cultures that are framed wholely in terms of the conceptual organization intrinsic to the cultures themselves.

CONTEXTS FOR CONCERTED BEHAVIORS

In Chapter 18, R. P. McDermott, Kenneth Gospodinoff, and Jeffrey Aron take up Frake's point that ethnographers can discover how society members formulate interpretations of behavior and experience by studying contextual frames. They argue that an adequate ethnographic account of "what is going on" at any given time must specify the contexts that society members use in organizing their behavior. Knowledge of cultural constructs and principles is essential for members and for ethnographers in order to perform appropriate behaviors; however, cultural knowledge can be used only in interactional environments,

or contexts, established for its use. Society members can interpret "what's happening" because they know which behaviors are appropriate in which contexts, and they know which contexts are relevant because they contribute to their construction.

The orientation of this chapter owes an intellectual debt not only to writers like Frake, Bateson, and Goffman, but also to researchers in the subfield of sociology known as ethnomethodology (cf. Sudnow 1972, Turner 1974). The program of ethnomethodology is to study the methods used by society members in accomplishing the social order, which is not reified as a material aspect of objective reality, but rather is viewed as a constructed aspect of social reality. And social events are not viewed as accomplished through the mechanical application of cultural constructs and principles, but rather as emerging through the cooperative efforts of participants who negotiate their realization in the course of social activities.

McDermott and his co-authors describe four ways in which society members establish contexts for their concerted behaviors: contexts are formulated—members use words or gestures to designate contexts; they are acted out—members organize their postures to indicate formulated contexts; they are oriented to—members' behavior reflects and preserves formulated contexts; and they are used—members hold each other accountable for behaving in accord with formulated contexts. Because each of these means of organizing contexts for concerted behavior is exhibited publicly by members, they are accessible to ethnographers as well as members. The principal aim of the McDermott, Gospodinoff, and Aron chapter is to describe in detail the methods used by members of society to frame contextually their concerted behavior and to show that those methods can be treated as criteria for judging the adequacy of ethnographic descriptions.

Positionings

McDermott et al. focus their attention on the positionings, or postural configurations, that six school children and their teacher assume during a reading lesson. They extensively analyze the "reading positioning," which is one of the positionings that occurs during the turns the children take reading their lesson book. More inclusive contexts (turns to read, instructional rounds, lessons) and lower-level units (actions that constitute positionings) are also described, but positionings are considered in detail because they are central in organizing the reading activity. Positionings embody the contexts that members use as the basis for accomplishing their reading behavior. In assuming and leaving positionings, participants express the current context and provide each other with the means for organizing their ongoing concerted behavior.

This is graphically illustrated by the authors in their analysis of a few seconds of videotaped behavior. In this segment of behavior, the children and teacher do not assume the postural configuration of the typical reading positioning, which is as follows: the teacher calls on one child to read and the children all lean down toward their books; the selected child begins to read aloud and the others listen or read along. In this particular behavior segment, the teacher calls on a child to read and the children, although they begin to assume the reading positioning, turn away and look around the room in different directions. As the authors show in detail in their chapter, this is a reading positioning despite its chaotic appearance. What transpires is an extremely precise two-

and one-half-second dance by way of which the teacher and children reorganize the reading positioning, without anyone uttering a word. Because the participants in the dance all know it is reading behavior in which they are engaged, they use certain movements (the reading child looks to the teacher to get her attention, the teacher turns to another child to bring him back to his book) to convey meanings to other participants and to hold them accountable to this formulated reading context. In this example, the teacher and children formulate a reading positioning, orient their behavior in terms of this formulated context and, by holding each other accountable to this context, reorganize their postures to reflect it. Positionings are thus contextual frames that members use to organize their concerted behavior and ethnographers can use to interpret behavior and experience in native terms.

CULTURAL SCHEMATA AND ACTUAL BEHAVIOR

Stephen M. Fjellman, in Chapter 20, describes another example of the interrelationship between cultural schemata and actual behavior: the interconnectedness of residence categories and actual residence choices in an Akamba community near Nairobi, Kenya. The aim in Fjellman's chapter is to formulate a decision model that represents the organization of Akamba cultural concepts in such a way that actual residences in the community are predicted by culturally defined social status characteristics (cf. Geoghegan 1971, n.d.). Census materials are the basic data used in constructing this descriptive model. These include information on household composition, present location of community members, sex, marital status and history, and so on.

In formulating his model, Fjellman uses these census data as background information to elicit ethnographic information on residence categories and social status characteristics. The technique he devised to do this is modeled on the familiar children's game Twenty Questions. Fjellman asked his informants to guess the mode of residence of unidentified people for whom he had census information, instructing them to ask him only yes/no questions to obtain the information they needed to guess the residence mode. The most efficient way to do this is by beginning with very general questions ("Is the person male?" "Is the person married?") and then asking increasingly more specific questions ("Is the person working outside the community?" "Is the person's mother married?"). In order to ask these questions, informants had to use the social characteristics that are criterial in Akamba residence decisions (for example, male vs. female, has been married vs. never been married, older son vs. youngest son, etc.) and the residence categories that are indigenous to the Akamba (for example, "stays with his parents," "stays with his son," "stays with his wife at his mother's," etc.). Fjellman was able to learn these cultural constructs from Akamba informants' talk about residence, and the account he constructs of Akamba residence is thus talk about talk about residence.

A Composite Model of Actual Residences

The composite model of the decision processes involved in Akamba residence, which Fjellman formulates on the basis of data from many informants, is a tree structure model. It has an entrance point and a number of pathways through decision points (represented as diamonds) that lead to particular outcomes. At

the entrance point (the leftmost diamond in Fjellman's Figures 3 and 4, pp. 409 and 410) all Akamba residences are divided into two sets, and at each succeeding decision point they are further divided until, at the end of the pathway, they are assigned to particular residence categories. The social status characteristics discovered through the Twenty Questions-type of elicitation procedure are represented as binary yes/no distinctions in the decision diamonds, and proportions of residences passed along from immediately preceding diamonds are indicated in parentheses for each yes and no decision. Because these parenthesized figures are proportions of earlier proportions, they are conditional probabilities. The final proportion at the end point of each pathway indicates the percentage of people in the entire sample who are predicted by the model to reside in the residence category.

The accuracy of the model can be tested by comparing these residence predictions of the model with the actual residences of the people in the sample. Fjellman's model is shown to be very accurate: it assigns 94.8 percent of the people in the sample to the correct residence category. Fjellman's model thus demonstrates that there is a highly patterned association between sets of social factors and residence categories. It represents the structure of the social status characteristics that constrain residence decisions. It also represents the decision process that operates on this structure to determine actual modes of residence. A detailed account of how decision-making is constrained by culturally defined characteristics of social contexts is thereby provided.

A COGNITIVE SYSTEM FOR LITIGATION SETTLEMENT

In Chapter 20, the cognitive system used in decision-making is analyzed by Naomi Quinn in her study of litigation settlement among Mfantse tribesmen in Biriwa, a town in Cape Coast, Ghana. Like Fjellman, Quinn focuses on cultural schemata underlying decision-making behavior, but unlike Fjellman she is concerned less with how decisions are influenced by culturally relevant features of social contexts than with how they are influenced by human cognitive requirements. The Mfantse litigation system is a natural, culturally instituted system: it is a system of shared knowledge that has developed to make naturally occurring (as opposed to experimentally induced) decisions. Mfantse litigation cases, except for the very serious ones that must be taken to the government courts, are settled by panels of elders, who hear the arguments of complainants and defendants and question them and various witnesses. Panels reach two decisions in private deliberations: they decide who is at fault, and they decide the fee that the offender must pay to pacify the offended party. The system used in making the latter decision, which Quinn examines in detail in her chapter, consists of a set of rank-ordered alternative pacification fees and a process for selecting the fees that are appropriate in specific cases. In this decision process, one rank-ordered alternative fee is selected as the departure point; from this point, moves up one rank or down one rank (and occasionally up or down two ranks) are then made at the suggestion of panel members, who state their reason for increasing or decreasing the fee (for example, the fee should be decreased because the case is between close relatives, because the offense was committed unintentionally, etc.).

As Quinn indicates, cognitive systems for decision-making and problem-solving

are not generally verbalizable. Arithmetic systems, such as the borrowing and carrying algorithms described in the following chapter by H. J. Reed and Jean Lave are exceptional in this regard. Unlike classification systems, these systems are not generally encoded in lexical items and are often extremely difficult or impossible to express in language. Quinn suggests that the structures and processes that comprise these cognitive systems can be determined using a method of concept formation: tentative principles are formulated and tested against the data until the principles that provide the best account of the data are discovered and then verified by informants. This technique and others that are currently being developed—an example is a technique recently described by Quinn (1979) for studying cognitive structures through metaphorical language—will unquestionable be important for progress in the study of decision-making, problem-solving, and other cognitive systems.

Simplifying Decision-Making

The properties of the Mfantse system for setting pacification fees that Quinn focuses on in her chapter are properties which simplify decision-making. One way in which the system simplifies fee-setting decisions is by reducing the facts relevant to the decision to three, four, or five, eliminating the need to consider all the facts involved in the case. Another way is by accepting or rejecting suggestions for increasing or decreasing fees one at a time, instead of by directly comparing all possible reasons for fee changes. Still another way the system simplifies decision-making is by being the accepted and agreed-upon means of litigation settlement. As Quinn points out, decision-making is a two-stage process—the first stage being the selection of a culturally shared knowledge structure and the second being the operation of cognitive routines on this structure. Therefore, the immediate availability of the traditional fee-setting structure for use in new litigation cases greatly simplifies the decision-making process and, thereby, the settlement of these cases.

What is most significant about these procedures for simplifying decision-making, Quinn suggests, is that they are adaptations to human cognitive requirements. The cognitive system for setting pacification fees is simplified so as to conform to the limits of human information-processing abilities. Selection of the structure of pacification fees (the rank-ordered alternatives) and constraints on the process operating on this structure (one-step and, rarely, two-step moves made one at a time) are basic features of the cognitive system for setting pacification fees that permit it to be used in making actual decisions. The major contribution that Quinn makes to our understanding of cultural competence, sociocultural performance, and their interconnectedness is her characterization of specific ways in which the cultural schemata underlying decision-making behavior are constrained by human information-processing limitations.

COGNITION AND BEHAVIOR IN ARITHMETIC PROBLEM-SOLVING

The next chapter investigates another cognitive system and its relation to behavior. H. J. Reed and Jean Lave, in Chapter 21, examine arithmetic problem-solving among tailors in Monrovia, Liberia, with the aim of achieving more than a

comparative account of problem-solving behavior: although they utilize comparisons at the performance level in their analysis, they are principally interested in gaining insights into cognitive structures and processes in problem-solving and how they operate in solving arithmetic problems. As the authors point out at the beginning of their chapter, arithmetic is a particularly suitable domain for investigating connections between culture and behavior because it is a highly systematic, formal domain about which a great deal has been learned and for which elaborate systems of mathematical notation have been devised (for another study of the arithmetic domain in Liberia, see Gay and Cole 1967; also see Cole et al. 1971).

The research reported by Reed and Lave was designed as a "naturally occurring experiment." By this they mean that they attempted to exert experimental control in a natural situation. The natural situation is Monrovia's Tailor's Alley, a block-sized area in which are located approximately 250 tailors and tailors' apprentices. The research was experimental in that it was designed as a controlled comparison of the influence of two types of educational experience, traditional tribal apprenticeship and Western schooling, on a single group of people, Vai and Gola tailors, who share a single culture (cf. Lave 1977). Different arithmetic systems are taught in the two educational settings, so the arithmetic problem-solving behaviors produced by tailors can be expected to vary, depending on their educational experiences.

Arithmetic Systems

The arithmetic tasks with which Reed and Lave are concerned were designed to study how four arithmetic systems affect problem-solving performance. As they define it, an "arithmetic system" is a cognitive system with two components, a set of number names and a set of strategies for performing the four arithmetic operations: addition, subtraction, multiplication, and division. Number names are symbols for discrete quantities; examples in English are the spoken and written word *twelve* and the written Arabic numeral *12,* both of which stand for the same quantity. Strategies for performing the operations are of two types: they either calculate quantities directly (examples are counting on the fingers, using buttons or pebbles, and marking tallies on paper) or they calculate quantities indirectly by using algorithms (mechanical calculation procedures) to manipulate number names (the example given by Reed and Lave is the algorithm used in English in adding numbers aligned in columns, "Put down the 2 and carry 1"). The four arithmetic systems examined in the chapter are the Vai/Gola and the school-taught systems and the monetary versions of these two. The basic contrast between the former two systems is that in the Vai/Gola system direct counting strategies are used in performing arithmetic operations, while in the school-taught system rote-learned algorithms are used. In the monetary versions of these systems, parts of the U.S. dollar (U.S. currency is used in Liberia) are the named entities *(penny, nickel, dime, quarter, half-dollar, dollar)* to which mathematical strategies are applied—direct counting strategies in the Vai/Gola system, and decimal point algorithms in the school-taught system.

On the basis of these very specific differences among the four arithmetic systems, Reed and Lave are able to make very specific predictions about the types of errors that will be made in using each of the systems. For example,

the use of the Vai/Gola system in adding discrete quantities should generate answers that are incorrect by small amounts because the addition operation is performed using counting and tallying strategies. On the other hand, the use of the school-taught system should produce answers that are too high or too low by 10 because the addition operation is performed using borrowing and carrying algorithms.

In order to confirm or refute their predictions, Reed and Lave adopt the following procedure: starting with the errors made by the tailors in solving test problems (and not knowing the identities of the tailors), they first predict which arithmetic systems were used in making the errors; they then check their predictions by examining data on the strategies used by the tailors while working the problems (whether they used a counting technique, a school algorithm, etc.) and data on the educational experience of the tailors (whether they had attended school, for how long, etc.). The arithmetic systems, error predictions, and tests of those predictions are considered in detail in the chapter, but it may be noted here that in general Reed and Lave's predictions are confirmed: their study of the arithmetic systems of Monrovian tailors very successfully illuminates the interconnectedness between cultural competence (underlying cognitive structures and processes in arithmetic problem-solving) and sociocultural performance (problem-solving behavior produced on the basis of these cognitive schemata).

COGNITIVE STRUCTURES AND BEHAVIOR ASSESSMENT

In Chapter 22, Roy G. D'Andrade considers another interconnection between culture and behavior, the relationship between long-term memory and the assessment of behavior. The point D'Andrade attempts to establish is that observers' judgments of the behavior of human subjects based on long-term memory (that is, judgments that are made at least ten minutes after the occurrence of the observed behavior) are systematically distorted, in such a way that behavior traits that observers conceive of as similar will be recalled as occurring together in the same subjects, even when in fact they do not. D'Andrade's hypothesis is that assessments of behavior are distorted by memory errors that reflect cognitive structures and are not accurate reflections of the actual behavior of human subjects in the real world. The ramifications of this distortion hypothesis are profoundly important both for our understanding of the influence of cognitive structures on behavior assessment and for our evaluation of the area of social science research that utilizes long-term memory judgments to discover multibehavior units—that is, constellations of traits representing the organization of subjects' behavior: for example, their personalities.

The major aim of D'Andrade's chapter is to test the systematic distortion hypothesis. To do this, he compares three types of data: data on the actual behavior of subjects, data derived from long-term memory, and data on the semantic similarity of behavior descriptions. The first two types, which are comparable since they were collected using the same system of categories for judging behavior—the Bales (1969) interaction-process category system—were obtained from two previously published studies. Behavior descriptions were selected to match the Bales categories, and judgments of similarity in meaning of these descriptions were elicited in tests devised by D'Andrade. Examples of behavior

descriptions are "shows solidarity," "jokes," "gives suggestions," and "shows tension."

Systematic Distortion in Behavior Recall

To test the distortion hypothesis, D'Andrade subjects these three sets of data to a "multitrait-multimethod matrix comparison," a statistical technique that compares the patterns of correlations occurring in separate sets of data. The major findings of this comparison support the systematic distortion hypothesis: the pattern of correlations in the data on actual behavior (immediately recorded judgments) is only slightly similar to the pattern of correlations in the data derived from long-term memory; by contrast, the pattern of correlations in the semantic similarity data is strongly similar to the pattern of correlations in the long-term memory data and not at all similar to the pattern of correlations in the actual behavior data.

There are several conclusions to be drawn from these findings. The first of these is that the systematic distortion hypothesis is confirmed. There is patterned distortion in the way observers recall behavior traits: it is more likely that they will recall sets of traits that are semantically similar than sets of traits that co-occur in actual behavior. So it appears that we can again conclude, as we did in the discussion in Section II of universality and relativity in color classification, that language serves as a vehicle for memory: in this case, relations of semantic similarity among behavior descriptions facilitate memory judgments of behavior. Cognitive structures encoded in language are a major determinant of memory, and in the case of judgments of what goes with what, can have an even stronger effect than what was actually observed.

High Frequency Behaviors

On the basis of D'Andrade's finding that memory-based assessments of behavior are distorted by the cognitive structures of observers, another very significant conclusion can be drawn: that social science investigations of behavior that rely on data derived from long-term memory judgments are doomed from the start. The multibehavior units posited in these investigations, being artifacts primarily of cognitive structures, do not shed any direct light on the organization of behavior and are, in fact, poor predictors of behavior (for discussion of this point, see Shweder 1979). An alternative behavior-specific approach, which D'Andrade describes at the conclusion of his chapter, provides a more direct means of predicting behavior. This approach attempts to account for the occurrence of specific behaviors in specific situations. A person's behaviors are rated on a number of classification systems (e.g., gestures, paralinguistic behaviors, choices of topics in conversation, etc.), and the most frequent behaviors in each classification are predicted to be the behaviors the person is currently performing. As D'Andrade states, extremely predictive descriptions of behavior can be produced on the basis of this behavior-specific approach. Earlier in this section, we considered research that attempts to account for specific behaviors in terms of features of the contexts in which they occur. For future research in this area, an even more powerful approach would be one that combines D'Andrade's high-frequency behavior analysis with the analysis of contextual factors.

DEDUCTIVE REASONING IN NATURAL DISCOURSE

In Chapter 23, Edwin Hutchins analyzes deductive reasoning in the natural discourse of Trobriand Islanders, a Melanesian tribal society that became famous in anthropology as the result of the early ethnographic studies of Bronislaw Malinowski, one of the founding fathers of modern anthropology. Malinowski's rich and detailed descriptions of Trobriand culture and society, published in the 1920s and 1930s, have provided many subsequent scholars with ethnographic materials for testing a variety of theories. One of these later writers, Dorothy D. Lee, used Malinowski's Trobriand materials to demonstrate that "primitive" thought is fundamentally different from modes of thinking in the Western world. Lee claimed, as Hutchins points out in his chapter, that the Trobriand language does not have terms for such semantic relations as cause–effect and temporal sequence, and that these relations are meaningless and totally irrelevant for Trobriand behavior. Lee's position is the by now familiar extreme, or complete relativist, position that we first encountered in Section II. In his chapter, Hutchins analyzes materials from his recent ethnographic fieldwork in the Trobriand Islands; he demonstrates that Trobriand reasoning is not "primitive" in the way Lee suggests but is very similar to Western reasoning. His conclusions indicate that the limited relativist position is again supported over the extreme relativist position.

Deductive reasoning is the cognitive process by means of which conclusions are inferred from premises. A classic example in Western logic is the Socratic syllogism. "Socrates is a man" and "All men are mortal" are the premises from which the conclusion "Socrates is mortal" can be inferred. In his effort to show that syllogistic reasoning is characteristic of Trobriand as well as Western thinking, Hutchins discusses the cultural premises used in inferring conclusions about Trobriand land tenure, and describes a land dispute case to illustrate Trobriand land tenure reasoning.

Land Tenure Reasoning

Because the premises involved in reasoning about land tenure are specific to Trobriand culture, an account of the conceptual organization of the land tenure domain is a necessary prerequisite to an account of Trobriand land tenure reasoning. The major cultural premise described by Hutchins is that for land tenure rights to be transferred the land must be *tupwa* or "still remaining" with the giver of the land rights at the time of transfer—that is, the giver must actually have rights in the land, and an exchange inducement called *pokala* must be provided to the giver by the prospective receiver of the land rights. *Tupwa* and *pokala* are specifically Trobriand cultural concepts; in fact, *pokala* is the unique beginner in a classification system that includes a number of subordinate categories of exchange inducements.

The land dispute that Hutchins describes is a legal case argued in the village court by two men who both claim rights to a piece of garden land. As will be seen in the account in Hutchins's chapter, the claims of both men conform to the logic of Trobriand land tenure. The court decision in favor of one of the litigants is determined on the basis of evidence supplied by witnesses, which indicated that the major cultural premise was valid in the winning litigant's

account of the land transfer, whereas it was invalid in his opponent's account. The conclusion, that the winning litigant had acquired rights in the land, is reached when it is determined that the land was *tupwa* with respect to the person from whom he claimed to have acquired it when he provided *pokala,* and that it was not *tupwa* with respect to the person from whom his opponent claimed to have acquired it when the latter provided *pokala.*

Reasoning in Discourse

Hutchins also demonstrates that deductive reasoning on the basis of cultural premises is used not only in determining court decisions, but also in producing and understanding natural discourse about court decisions. He presents a transcription of the court's explanation of the decision to the losing litigant, and shows that twelve inferences based on the major cultural premise are needed to understand this brief discourse. As Hutchins's analysis makes clear, the interpretation of this discourse involves a great deal of complex reasoning, not the least of which stems from the fact that the cultural premise on which this reasoning is based is generally only implicit in the discourse.

As Hutchins argues, reasoning in the Trobriand Islands is conducted in much the same way that it is in Western societies. What is different is not the cognitive process of inferring conclusions from cultural premises but rather the cultural premises themselves. The cultural knowledge about the world of experience and imagination included in such premises is specific to individual cultures and differs widely from culture to culture. In Trobriand reasoning, then, there are two sorts of cognitive schemata—a universal process and culture-specific structures—underlying the sociocultural performance of court decisions and natural discourse about court decisions.

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17

Plying Frames Can Be Dangerous: Some Reflections on Methodology in Cognitive Anthropology

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(What is written here is not so much a "paper" addressed to a reader as it is a representation of a "talk" presented to an audience. Some of the discussion refers to the audience's shared experience as listeners to the talk. It would be helpful if the reader could imagine that he or she were a member of that audience. Accordingly, he or she will henceforth be addressed in the second person. Remarks addressed to you as a "reader" rather than as an "audience member" will be set off by dots and placed in parentheses.)

* * *

Cognitive anthropologists,[1] sometimes known as "ethnoscientists," are said to be people who listen to what the natives have to say. I do not claim to be one of the experts among cognitive anthropologists, but I do claim to be a native. This occasion, then, can be taken as an opportunity to assess the interest of listening to a native's point of view about the activities of his own group.

The research tradition that has come to be known as "cognitive anthropology," like most labeled schools of thought, includes such a diversity of approaches and perspectives that it is difficult to find much, apart from the label itself (and, maybe, the attacks of Marvin Harris), that holds the tradition together. Probably the most apparent common theme has been a concern for methodology, inspired by an admiration for the supposedly greater rigor of sister sciences, either psychology or linguistics. The value of the various methods proposed may be debatable, but the effort of pursuing them has succeeded in giving the field of cognitive anthropology a bag-of-tricks image among both adherents and critics. Some adherents seem to feel that the problems of the field, problems of lack of coherent theory or of substantial descriptive accounts, can be solved by ever more diligent pursuit of new methods. Somehow, with tighter frames, more dimensional scales, and more flawless flow charts, the cognitive maps of our informants will be brought into focus. Critics, on the other hand, have pointed to the danger of this kind of excessive methodological tinkering. The scope of the data narrows to accommodate the methods. A pursuit of methods that work for something—anything—replaces the search for a theory that explicates what we want to know.

I am not about to suggest that cognitive anthropology abandon its methodological concerns. I do not recommend following the route of some linguists, freeing oneself for theoretical flights by cutting off ties

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with empirically grounded data. Nor do I advocate joining the hermeneutic circle, burying data under repetitive interpretations of "what it means (to me)," a tactic that produces thick books but does not necessarily deepen understanding.

Methodology, some theoretically motivated notions of *what to do* when faced with the real world, is as necessary in science as it is in everyday life. Methods link data—what we construe to be observations of some particular reality—with theory, our proposals for understanding reality in general. When methods fail, the answer may be not only to tinker some more with the methods, but also to rethink the theory. My purpose here is to reflect upon some of the methodological successes and failures of cognitive anthropology in terms of their implications for general conceptions of the relations among behavior, verbal descriptions of behavior, cognition, and culture.

I will focus on what is certainly one of the best-known items in the cognitive anthropologist's bag of tricks: the frame. This methodological device was lifted out of the distributional model of structural linguistics, and shares kinships with similar notions of the same ancestry: paradigmatic/syntagmatic, slot-filler, contrast/contiguity, alternation/co-occurrence. Some element, A, is specified by its contextual constraints, X—Y, and by its relation to other elements, B, that can occur in the same context, $X\left|\begin{matrix}A\\B\end{matrix}\right|Y$. The unique, and still poorly appreciated, contribution that the cognitive anthropologist made to this contextual model was that the context was not limited to portions of single, isolated sentences. A frame was construed as an inquiry matched with a set of responses. The unit of analysis was a question-answer sequence, a conversational exchange.

This extension of the range of linguistic context beyond single speakers uttering isolated sentences was made in an effort to find a context that would frame semantic, rather than grammatical, relations (the latter being the sole concern of both structural and transformational linguists at that time). Inquiries specify informational contexts, constraining the semantic domain of the response. Speakers of language were seen as question askers and answerers, not simply as sentence producers. This pursuit of meaning by relating sentences produced by different speakers together as part of a discourse was an advance over the sentence-bound semantics practiced until recently by linguists. (Compare the analysis of "bachelor" in Katz and Fodor's much-heralded 1963 paper on linguistic semantics with what was being done at that time in ethnographic semantics. Katz and Fodor's analysis would have allowed a married man to answer a woman's question "Are you a bachelor?" "Yes," on the grounds that he was a holder of a B.A. degree.)

The notion of frames proved to be a powerful and useful methodological tool. It provided ways of obtaining and organizing certain kinds of data so that they made certain kinds of sense in convincing ways. But, as critics have been quick to point out, not all kinds of data proved to be equally tractable and, more significantly, the results, while they may have made some kind of sense, often did not seem to answer very interest-

ing or important questions. In contrast to the essays of symbolic anthropologists, not so hampered by methodological constraints, the output of cognitive anthropology often seemed compartmentalized and trivial. One might counter that one person's trivia is another's eureka, and, moreover, a secure little truth is as useful as a wobbly grand theory. Nevertheless, cognitive anthropology ought to aspire to bigger truths, to go beyond offering tiny fragments of cognitive maps from here and there, to offer an overall view of the landscape.

More upsetting to cognitive anthropologists than the triviality issue have been problems of inducing people to verbalize in consistent question-response fashion about many topics of interest to the investigator and of obvious relevance to the people being studied. Why is it easier to get a taxonomy of birds than of social roles?

The other problem that has arisen from applying frames and other, more experimental, methods in cognitive anthropology has been the high degree of informant variability that is so often manifest. This result, to my mind, reveals a strength, not a weakness, of the methods. It reflects the way the world is, a reality less methodologically oriented approaches in anthropology have obscured. The last thing we should do is to flee from this reality or to tinker with our methods to eliminate it. But there remains the question of how to account for variability. The traditional use of the frame as a question-response device leads to what I think is the wrong answer: that we each go around with unique cognitive ideolects in our heads, each of which much be separately described and somehow summed up to equal culture.

These methodological difficulties have arisen from a failure to exploit fully the interactive aspects of the frame model, to widen the frame so as to capture a context that more fully specifies how human behavior comes to have meaning. Instead, attention was focused on questions and responses as chunks of verbiage isolated from their settings and their speakers. The specter of the stimulus-response model of behavior hung over many early programmatic statements. Some investigators emphasized that the idea was to *discover* the questions (stimuli) that evoked the answers (responses) we were trying to describe. But this notion that the answers are there, that the job is to find the questions, while often cited, did not seem really to take hold. Frames began to be called *eliciting* frames, to be thought of not as contexts for behavior but as prods to behavior. The ethnographer, rather than the informant, thus becomes the questioner.

Of course, one tries to elicit the questions from the informant, but this process can amount to little more than finding out how to translate into the informant's language the questions the ethnographer wants to ask. Both the prevalence and the hopelessness of this procedure have become apparent to me in classroom informant-eliciting exercises, both those I have staged and those I have witnessed. In cases where I had some knowledge of the language and culture (and I quickly restricted this game to such cases), it was clear that the only way to discover useful questions was to specify inquiry contexts within which such questions could be asked. Doing this in English without a knowledge of the culture

is nearly impossible for most domains. But it does show, more clearly than actual ethnography (where the context is more likely to be taken for granted because it is there) that questions have to be related to larger contexts.

Apart from the distortion of frames into probes, there are certain technical difficulties with the notion of a "question." And inquiry for information (a query) is a kind of speech act that must be distinguished from a question, a grammatical interrogative. An interrogative can, and perhaps most often does, represent such speech acts as summons and greetings, which are not queries. The ethnographer of American disease who goes around our society asking "How are you?" is not likely to elicit a very large inventory of disease terms. In Yakan (a Philippine language), a frequent question is "Who is your companion there?," an ideal question, one might think, for eliciting terms of social identity. Yet, the question is most appropriately posed to someone who is alone. The only appropriate answer is "nobody." This question is, in most contexts, a greeting. The Yakan question "What are you carrying there?" is typically a greeting if what is being carried is easily visible, a query if it is not (Frake 1975). The status of a question as a query is dependent on the context in which it was uttered. Formal eliciting—so-called white-room ethnography—is an attempt to circumvent this problem by removing all previously relevant context, training the informant to see the white room as an interrogation chamber. This is an excellent methodological strategy—if what we want to know is how people behave in white rooms.

Even when we have a context in which we know that a given question is a query, we still can't be certain what query the question represents. A single question (a given surface-structure form) can represent a variety of queries for different kinds of information. The form of the question constrains the grammatical form of the response, but it does not, in itself, necessarily constrain the semantic domain of information. In Yakan, the common question "X is Y's what?" represents any query, the answer to which can be given in the genitive (surface) case: X is Y's grandfather; X is Y's rice field; X is Y's roof; X is Y's fate; etc. The answer to this kind of problem is not to search for more specific and necessarily highly artificial questions that, it is hoped, will sort out these different semantic relations, but to attend to the wider contexts of questioning that accomplish this sorting for the Yakan.

If one takes seriously the admonition to go out into the real world and look for queries, to seek "query-rich settings," as I once put it, one finds that people talk all the time and ask each other a lot of questions, but disappointingly few of the questions represent queries about the overt topics of the questions. Even children, the champion questioners, use this grammatical form in subtle ways. The child's stock question, "Mommy, you know what?" is not a request for information, but a clever use of sociolinguistic rules to acquire speaking rights (Sacks 1972).

Perhaps instead of trying to devise provocative questions and other instruments to persuade people to talk about things they do not ordinarily talk about in that way, we should take as a serious topic of investigation what people in fact talk about, or, better, what they are in fact doing

when they talk. When we look at talk, we find that people do not so much ask and answer inquiries; they propose, defend, and negotiate interpretations of what is happening. Because what is happening is what we are interested in explicating, these interpretations provide the key to understanding. Viewing informants not just as question-answerers, but also as interpreters of their lives, provides not only a sounder perspective for handling problems of informant variability and reticence, but also a more realistic notion of the relation of cognitive systems to behavior.

It is not so much that some things are hard to talk about. People can and do talk about anything. But some questions, if taken seriously as inquiries, are hard to answer. What kinds of sounds are there in your language? This is obviously a ridiculous question to pose to an informant if what you want to know are his phonological concepts; yet it can be answered, not by asking it, but by attending to interpretations of sounds made by speakers of the language. The problem with verbalized interpretations is not a difficulty in eliciting them but in locating what cues are being responded to in formulating a particular interpretation. Cues of sound, appearance, expression, body stance, and movement often cannot easily be explicitly identified by those who use them. Careful observation of the behavior, object, or event being interpreted is required. Simply recording what people say about things is no more adequate than simply recording what one sees. The informant's interpretations must be linked with the investigator's observations.

Attending to interpretations will not eliminate variation, but it will help to explicate it. Of course, people vary in behavior because they have different life experiences, different childhood traumas, different mental capacities, different hormonal balances, and so on. But this is only part of the story. Informants vary in what they say and do because interpretation itself is problematic. (It can be especially problematic when an informant is confronted with an ethnographer across a tape recorder.) An interpretation is not an answer to a question automatically produced in the mind by a cultural computer program as a result of proper input. It is a proposal, a theory to be tested, tested not only against the reality it covers, but (like scientific theory) also against its reception by one's fellows (or by the ethnographer).

Construing talk about things (including responses to the investigator's queries and tests) as proposals for interpreting not only what is being talked about, but also what is going on *now*, makes variability in verbal responses much more understandable. Where we must seek underlying cultural constants is not in the content of the talk, but in the principles for formulating interpretations, for making sense of life. It is when things do not make sense that you know you have wandered off the edge of your cognitive map.

My arguments thus far all point to the necessity of expanding our frames to encompass the wider social context that makes interpretation possible. Calls for considering wider context, for defining behavior in terms of the situations in which it occurs, are certainly not new. Malinowski made them, ethnoscientists and ethnomethodologists have made them, sociolinguists have made them. Even straight linguists have begun to

make some moves in this direction. Appeals are made, but it is rarely very clear how one specifies and delimits relevant context. All that is clear is that specification of relevant context is problematic, not only for investigators, but also for natives. It is itself, as ethnomethodologists are fond of telling us, a matter of interpretation. Context is not there to be seen. Its specification is a social accomplishment.

One way to begin a search for the units by which the specification of context is accomplished is to track a bit of meaningful behavior through a variety of native interpretations. Take one of the more secure findings of cognitive anthropology: that the English word for *mother* is "mother" (sometimes pronounced "muh-thuh"). Cognitive anthropologists have learned this by asking a query equivalent to "How are you related to so-and-so?" where the so-and-so's are named individuals in the informant's social world. This frame serves to sort out a domain of "relatives" ("We're not related; we're just friends") and also to distinguish real "mothers" from such metaphorical mothers as mother superiors, mother tongues, and mother nature—the kinds of mothers who attract symbolic anthropologists.

Componential analysis permits us to define real kinship mothers as female, first ascending generation, lineal ($♀G^{+1}L$). Whenever you encounter a $♀G^{+1}L$, you have found yourself a real mother. But even real $♀G^{+1}L$ kinship mothers can suffer a variety of interpretive fates. Here are some recorded comments on mothers, made by a native—an American-English-speaking informant of Irish-Catholic background. The informant begins by describing his home community.

* * *

(In the oral presentation of this talk, I play here a few excerpts from a phonograph record by the comedian George Carlin [1973]. He does a routine on his neighborhood, "White Harlem" [Morningside Heights], then one on verbal dueling, "slip-fights," in the parlance of his group. He notes that some groups have a rule for slip-fights: "No mothers, man; no mothers." His group didn't have that rule. They started right in with mothers:

"Hey, where'd yuh go last night?"
"I was out with yuh muh-thuh, man."

He then notes that it is a cause of some embarrassment if the mother of the addressee turns out to be dead:

"I forgot, man."

Carlin goes on to acknowledge the origin of slip-fights in such Black street games as "the dozens." He recites an example of the dozens, in which the reference to what the speaker does with the addressee's mother is rather more graphic than in the white, Irish-Catholic slip-fight example. The recording includes the laughter of Carlin's audience.)

* * *

The slip-fight exchange is ostensibly rather innocent. It is not difficult to imagine contexts in which it could so be taken: if the respondent, for example, were the questioner's father. There could also easily be contexts in which this exchange could be a grievous insult: if, for example, there were good reason to believe that the respondent really had been

out with the questioner's mother. But what we have is not a real insult, but a ritual insult as part of a game of verbal dueling. But if the target's mother happens to be dead, then the insult can no longer be taken ritually. An apology is called for. So, even though participants clearly take the depicted event as mythical, it is a real kinship mother, not a mythical or metaphorical mother, who is being referred to.

But, of course, what *you* (the member of the audience) heard was not the mention of "mother" in a slip-fight. Nor did you really hear an informant describing a slip-fight for the enlightenment of an ethnographer. I am sure you quickly saw through that fabrication and realized you were hearing a performance intended to entertain an audience. So you knew you were, not being insulted, not even ritually. But you were not being entertained, either. For you are not an audience of a "show" but of a "talk." And, although the audience of a talk is allowed to laugh, too, we would all agree that reference to mothers in this talk would be a failure if it could not somehow be interpreted as a reasonably apt illustration of some serious, scholarly point.

The point here is not so much to characterize varieties of such speech acts as insults and invitations. This is an active enterprise now in several scholarly fields. Nor is the real point even that of Goffmanesque frames (Goffman 1974): how to distinguish real insults from ritual ones in slip-fights; real slip-fight ritual insults from performances of them in a show; and real show performances of ritual insults from illustrative use of them in a talk. These matters are very relevant. But my focus here is not on the kinds of acts that can be discerned within contextual frames, nor on the human capacity to reframe reality repeatedly, but rather on the shape of the contextual frames themselves, frames within which people organize their conceptions of what, basically, is happening at a given time.

It is easy to argue for the indeterminability of specifying what is really happening at any given here and now. What is the spatial extent of here? What is the time span of now? Which of the multitude of detectable motions and changes surrounding us and within us constitute what is really happening here and now? Are we scratching our heads? Feeling hungry? Worrying about taxes? Watching a fly on the wall? Breathing in our neighbor's cold germs? Being bombarded by cosmic rays? Suffering through a drought? Being watched over by the gods? Traveling around the sun on a whirling ball? Actually, we all know very well what is happening now: this is a talk. That this is a talk structures the time and space of what's happening. Within these boundaries of here and now, whatever transpires is interpreted in relation to our shared awareness of what we are doing. You may not be paying attention to the talk—your thoughts may be far away from this time and place—but what you are doing is not paying attention to the talk. And it is only by knowing that this is a talk that you know what you are doing.

A talk, like a slip-fight and a show, represents the kind of thing we are looking for, a basic unit of interpretive context. At a rather immediate level, the kind of thing a talk is might be termed a social occasion. Social occasions are relatively easy to recognize. They are where ethnographers

go for their data: to weddings, parties, ceremonies, legal cases, etc. Typically, investigators intrude upon an occasion, a religious ceremony for example, collect samples of behavior within it, and then go home, examine the samples, and write a monograph on religion. But the nature of the occasion that made the behavior "religious" for the participants is often ignored. Making the nature of occasions themselves an object of study has not been entirely neglected, however. A number of studies in anthropology, sociology, and sociolinguistics could be cited.[2]

Some of my work in this area has been guided by an image of a society as an organization for the production of social occasions, or "scenes," as I have called them, and of a culture as a script for planning, staging, and performing scenes. The major chunks of society, institutions like "religion" and "education," can be discerned and defined with respect to particular social systems—"emicly," if I dare use that word anymore—by viewing them as organizations for the production of scenes of the same type. In our society, "education" can be seen as a complex of buildings, teachers, bureaucrats, procedures, values, etc., which have the purpose of assuring that classes and such related occasions as office hours and academic talks take place.

Of more interest for cognitive studies, however, is that, by attending to the way occasions are contrastively defined, classified, distributed among settings, scheduled, and linked by planning sequences, this and related approaches have begun to reveal dimensions of cultural structure that do promise to give overall views of a culture's conceptual landscape, tying together the fragments of cognitive maps left behind by previous studies. These dimensions provide an alternative to the traditional institutional rubrics of religion, education, economics, politics, law, etc., which serve to divide up the academic world and organize monographs, but, perhaps, do not always reflect how people divide up and organize their own cultural worlds.

Such dimensions of cultural structure begin to emerge when one considers what makes a talk a talk and not, say, a class, or a speech, or a show. We can note that what goes on in talks and classes is to be taken as instructive, whereas shows are entertaining and speeches are persuasive. Talks can be distinguished from classes by (among other things) role structure: talks have a "speaker" and an "audience"; classes have a "teacher" and "students." The differences are more than terminological. For one thing, speakers get introduced. And that little difference is a marker of a very important dimension of cultural structure found, I would argue, in all societies: the dimension along which some occasions are marked as more special than others, as requiring more planning, as having explicit signals of beginning and ending, as entailing elaborate and explicit rules of procedure, and as demanding marked behavioral displays—dressing up, sitting straight, and speaking in elevated style. This is the dimension of formality, the dimension that occurs so often as a context for linguistic variation.

This dimension has interesting links with social stratification, in that everywhere displays of formality—like a necktie, erect posture, long sentences, Latin words, titles before names, and, among New Yorkers, postvo-

calic r's—are also emblems of high status and, at the same time, a display of deference. So my necktie may be taken as a tribute to this occasion, or a badge of my middle-class standing, or a sign that I have just come from the Dean's office. (Then, again, I may have worn a necktie, a very unusual garment for me, simply to illustrate this point.) Cultures differ, of course, in how they range social occasions along the dimension of formality. Classes and talks are probably more formal in France than in California.[3] These differences can have important implications for the investigator's interpretation of what a particular kind of activity, say litigation, does in a given society (cf. Frake 1972).

Societies also differ in the value they place on formality itself as a symbol of identity and differentiation. Some peoples think of themselves as refined and civilized, rather than crude and vulgar; others, like Americans, and especially like western Americans, pride themselves in being natural and casual, rather than artificial and stuffy. These kinds of differences are not limited to the Western world. The Batak and Javanese of Indonesia provide a classic case (Bruner 1974). In our own society, we also think of previous eras as being more formal than our own. Things seem to get more informal all the time (I can remember when I wore neckties to classes, as well as to talks). Why does formality become a symbol of ethnic and historical differentiation? I do not intend to pursue such questions here, but wish only to indicate some of the directions in which the study of social occasions in their own right has led.

There are other general dimensions like formality—"risk," for example. Assessment of risk can operate in rather subtle ways, even in relatively nonrisky situations. One way in which talks differ from classes is that, in a talk, the speaker faces some risk, a similar risk to that confronting a performer in a show; whereas the audience, you people, can relax. In a class, on the other hand, it is the students who typically face the risk. Facing risk is a test of character. Colloquial English provides a rich vocabulary for evaluating performance in this test: he kept his cool, he pulled it off, he got by, he blew it. Many societies stage risky social occasions for this purpose, for providing some "action" (Goffman 1967). Among many groups—Chicago street gangs, for example—the asessment of risk provides the primary dimension for distinguishing social occasions, for differentiating "humbugging," "gang-banging," "wolf-packing," and "hustling" from "gigs," "games," "sets," and "pulling jive" (Keiser 1969). It is in the former set of risky activities that one earns "rep," displays "heart," and shows he is no "punk."

Now let me move on from dimensions revealed by contrasting types of social occasions to the kind of thing a social occasions is. Consider again this talk. What most of you probably planned to do this evening was to come to a talk rather than, say, to go to a show, watch TV, read a book, kill time, or find some action. You probably did not plan to "do education." Nor, at the other end of this continuum, did you plan to sit in a chair in this lecture hall—although that is what a camera would reveal you are doing. But it is not what's happening. Later, if asked what you did this evening, saying you "went to a talk" is an appropriate account. To say that what you did was to sit in a chair in a lecture hall

would sound odd, unless you could propose a context, a happening, within which sitting in a chair becomes eventful. "I was sitting in this chair and suddenly the roof collapsed over my head." What is happening is no longer a talk, but an earthquake.

A talk, then, exemplifies a conceptual unit whereby we organize our strips of experience in formulating accounts of what is happening, our memories of what has happened, and our predictions and plans for what will happen. Let me call such units "events." Note that "events," in this sense, are not occurrences out there in the world capturable by a camera, tape recorder, or behaviorist observer. They are proposed interpretations of what is happening at some time and place.

A talk is a social event, an occasion. But there are other kinds of events. Some things are not planned for and staged; they happen to one: earthquakes, droughts, illnesses, wars, gas crises, flat tires. Of course, in many societies it is a matter for some discussion whether such things just happen or are the deeds of motivated agents; but in any case, unless one is a malicious god, a witch, or a plotting general, such things are not, for most of us, social occasions that we plan and stage. They are happenings that befall us. Like social occasions, happenings provide interpretive contexts for behavior and units for formulating accounts. Ordinarily, in the polite society of Marin County, California, it would be unthinkably crude for a dinner guest to ask his host if he should flush the toilet. Now, in the context of what's happening being a severe drought, such a question is a sign of gracious consideration. Toilet-flushing and other ordinarily mundane hydraulic tasks have become eminently reportable topics of conversation and favorite subjects for newspaper columnists.[4]

Every situation in life, as it is experienced, can be defined by reference to one or more events that can be construed to encompass it and to lend meaning to what occurs within it. We account for our lives as sequences of eventful chunks of experience. Of course, not all experience is equally eventful. Sometimes nothing much happens. There are occasions when one is hard put to think of interestingly reportable occurrences to fill in conversations, letters, diaries, and field notes. To assert the occurrence of a particular event is to propose something significant and reportable about the experience it encompasses. As a proposal, it is subject to test not only against the reality it covers, but also against its reception by one's fellows.

The view proposed earlier, of culture as a script for the production of social occasions, should be recast a bit into one that sees it as a set of principles for creating dramas, for writing scripts, and, of course, for recruiting players and audiences. Culture provides principles for framing experience as eventful in particular ways, but it does not provide one with a neat set of event-types to map onto the world. Culture is not simply a cognitive map that people acquire, in whole or in part, more or less accurately, and then learn to read. People are cast out into the imperfectly charted, continually shifting seas of everyday life. Mapping them out is a constant process resulting not in an individual cognitive map, but in a whole chart case of rough, improvised, continually revised sketch maps. Culture does not provide a cognitive map, but rather a

set of principles for map-making and navigation. Different cultures are like different schools of navigation designed to cope with different terrains and seas. In this school, one must learn not only how to map out everyday life, but also how to fix one's position, determine a destination, and plot a course. And because people do not voyage alone, one must recruit a crew. Maps, positions, and courses must be communicated and sold. The last time—on a real boat in a real sea—I tried to sell a position and course to my crew (which included a distinguished cognitive anthropologist), I won the argument but promptly ran the boat aground.

NOTES

1. Presented as a talk to the Institute of Human Learning, University of California, Berkeley, April 1977. The talk was a revision and expansion of a presentation made under the same main title to a Conference on Cognitive Anthropology at Duke University, 1974, organized by Naomi Quinn and Ronald Casson. The appearance of this version in print owes much to the prodding and encouragement of Ray McDermott.
2. See especially the works of Erving Goffman and of the ethnomethodological sociologists. Useful recent studies by anthropologists include Agar (1974, 1975) and McDermott (1976).
3. Even French elementary school classes strike Americans as rather formal; see the classes so remarkably portrayed in Truffaut's movie "Small Change."
4. In the 1974 version of this talk, I used the then ongoing gas crisis to make this point. In the current era, there always seems to be some ambient crisis we can orient to.

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18

Criteria for an Ethnographically Adequate Description of Concerted Activities and Their Contexts

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The object of any ethnography is to describe some people's activities and to locate these activities within the various contexts for their occurrence. There is a requirement, often neglected, that such a description of behavior and its contexts be presented in a way that readers can decide for themselves whether or not to believe the ethnographer's account of what it is that a particular group of people is doing at any given time.[1]

In this paper, we will offer criteria for judging the ethnographic adequacy of any description of concerted behavior, and we will apply these criteria to a film of the activities of a small group of children and their teacher during an 11 minute reading lesson in a first grade classroom. A fine grained analysis of one three-second strip of behavior will be used to highlight the adequacy of the description of the lesson. This strip of behavior has been described as chaotic by many viewers of the film. By pointing to the order in this apparently chaotic behavior, we will raise the possibility that most behavior is ordered in ways about which we as observers or participants are systematically inarticulate. By implication, we will ask the reader to worry about the adequacy of any description of activities that does not meet these criteria.

A fundamental assumption of this paper is that there is a close similarity in the primary problem facing ethnographers and other persons engaged in everyday life. This problem, common to both, is the necessity of achieving a *working consensus*[2] with others about what is going on in any scene available to their senses. This identity is generally unrecognized. It usually is reasoned that ethnographers must describe the structure of social action in a way which is articulate only to other ethnographers and adequate to predict some next set of native actions. Natives, on the other hand, are less constrained to articulate the structure of their behavior (or more

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† Source: Semiotica 24(3/4): 245–275, 1978 (revised).

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constrained to be inarticulate about what is going on) and are more immediately constrained to achieve a communicational consensus long enough to maintain, change, or walk away from it. Nonetheless, enthnographers and natives share an identical concern. The ethnographer's adequate account of what natives do together must follow from the way in which the natives structure a situation to allow their participation with each other from one moment to the next. The ethnographer must articulate the same hesitant and momentary contexts that the natives are displaying to each other and using to organize their concerted behavior.[3]

The warrant for setting such a difficult goal for ethnographic description is that people manage concerted activity only by constantly informing and conforming each other to whatever it is that has to happen next. It had been thought, instead, that people managed concerted activities simply because they know how to do them. Knowing how to do things is crucial to the doing of them, but there is more involved in completing any activity; knowing how to get from Osaka to Kobe will not get you there. We allow ourselves the illusion that knowledge is the only necessary ingredient in social action, for we all make good guesses about what is going to happen next in social interaction. But we ought to acknowledge that our guessing percentage is directly related to the fact that each of us has a hand in producing the contexts in which we test our hypotheses about what is going on. Our knowledge turns out to be useful only after we have helped to set up conditions for its use.

People manage to know for sure what is going on only in relation to specific interactional environments, and they usually know a great deal about these environments because they have helped to construct them as predictable contexts for plying whatever it is that they know. Such framing takes considerable work, and it is by that work that we are able to locate what it is that members of a group are up to at any given time. *We can use the ways members have of making clear to each other and to themselves what is going on to locate to our own satisfaction an account of what it is that they are doing with each other.* In fact, the ways they have of making clear to each other what they are doing are identical to the criteria which we use to locate ethnographically what they are doing.

I. THE CRITERIA FOR DESCRIPTIVE ADEQUACY

There are four aspects of the work people do with each other to display a context in terms of which they can organize their next activities. These are not four different kinds of behavior, but often four different aspects of the same behavior. Contexts are: (1) in some way, formulated by the members of the group, in words or gestures; (2) in some way, usually by postural positioning, acted out in form as well as content; (3) behaviorally oriented to as patterns by the members at certain significant times; and (4) used by the members to hold each other accountable. We will elaborate each of these points and then illustrate their use in an analysis of some behavior.

(1) *Members usually reference or in some way formulate some of the*

contexts for their behavior. Members name or otherwise reference a context *in situ.* We cannot reply on people to name directly the most immediate contexts for their concerted activities. Although they may talk in terms of major events or reportables, they also act in terms of slightly smaller units which are not likely topics of conversation (Frake 1975, 1976). We will have to be sensitive to some of the less obvious formulations in terms of which people struggle to keep themselves informed of what they are doing together. The concern of this criterion is whether people are able somehow to formulate their behavior as a particular kind of behavior which they can be expected to recognize upon its reference as a possible context of the moment.

In addition to naming, a context can be formulated by a statement of what is required of a member in a particular context. For example, the structure of a classroom reading lesson can be formulated by the teacher calling on a child to read or by a child complaining that he never gets a turn. Such clear signals are usually used at the beginning of a behavioral context or when something goes awry. Thereafter, either through the duration of the context or on repeated occurrences of the context, the signals can be considerably shortened; precise verbal formulation can be replaced by a no less precise, but far simpler head nod or hand gesture.

On occasion, a gesture can stand on its own as the sole signal for a prolonged context. The condition for the description of any signal as a feature of the work members do to contextualize each other is that it is responded to; that is, upon signaling the members act as if the signaled context was in fact the reality at hand, be it accepted or challenged.

We have limited our discussion here to fairly obvious formulations as announcements of a context at hand. However, recent work in conversational analysis indicates that formulations of what is happening at any given time are ongoing, that a single formulation lasts about as long as its constitutive utterance and must be quickly restated in order to act as an organizing device for the members' construction of some next strip of concerted activity.

Sacks (1974) has offered us an important example of how people constantly formulate the contexts of their behavior in his analysis of the telling of a dirty joke. Although only one person is telling a joke to two others, the joke's telling takes some 85 different speaking turns on the parts of the three participants. Throughout these exchanges, Sacks is able to show how the members both use and contribute to the formulation of the context for their behavior as both a telling of a joke and a telling of a dirty joke in particular. Formulations are used to describe the moment at hand and reflexively to organize the context in which next actions take place.

(2) *Members usually organize their postures to form a configuration or positioning which signals the contexts for behavior.* In other words, people usually take on postures which are indicative of the task before them, and they usually do this together. This criterion is essential to the structural description we will offer of the behavior of the reading group members. We must be careful, however, to remember that these

positionings do not follow one upon the other with identical form and according to a schedule fixed by an exact calculus. Each positioning represents a struggle during which members negotiate their relations with each other. Every person involved carefully monitors everyone else for an interpretation of what it is that is going on. Thus, people use their positioning of each other to guide their responses to each other, just as they use their own formulations of what they are doing to guide their concerted behavior. It is precisely for this reason that we can use people's formulations and positionings to judge the adequacy of our description of their activities. In making their sense of what they are doing together visible to each other, participants to an interaction make their behavior available to the close scrutiny of an analyst. Thus, we should be able to use members' formulating and positioning work as a warrant for saying precisely what they are up to.

(3) *Members behaviorally orient to the order in their concerted behavior and accordingly constitute and signal their contexts for each other.* While formulating the positioning according to the most pressing context at hand, members are careful to orient to each other's doing or not doing of the formulated order. Within any interaction, there are some significant moments during which the participants must struggle to preserve the order of their relations. At these special moments, we can locate the concerted attention patterns which make a context what it is. In people's orienting work at these special times, the group's working consensus being serviced by their concerted activities becomes visible.

Sacks (1970) has given the following example which should make this criterion sensible. An American conversation is marked by not more than one and not less than one person talking at a time. Analytically, this sounds like a preposterous working definition, as it is clear that in most conversations there are occasions when two or more or no one will be talking. But Sacks is not interested in a definition of a conversational context simply in terms of a count of who talks and when, just as we are not interested in a definition of a postural context for behavior solely in terms of who moves and when. Rather, his effort is to locate rules people are using to direct conversational behavior. In other words, he is searching for a definition of a conversation as a context, as something that people formulate and act out in observably precise ways. Now we can attend to our third criterion for an adequate description of a context. Sacks' definition of a conversation is, in fact, the definition of a conversation as it is operated on by talking people. In most American contexts, anyway, when more than one or less than one person is speaking at a time, participants in the conversation behaviorally orient to the break in procedures. Consider only the uncomfortable feelings most people experience in a lag in the conversation, or the disruption caused by one person constantly interrupting others. At such times, people look away, fiddle with their hands, or simply leave the scene. With such activities, people demonstrate that their definition of a conversational context is behaviorally oriented to and as such constitutes part of their reality.

In using their own formulated and positioned order as a guide to subse-

quent behaviors, there are two kinds of special moments to which most members attend carefully in any interaction.

(a) Transition points are crucial. The shift from one positioning to the next is carefully oriented to, and it is during this time that the members attend to the shared aspects of their realities and move on them in concert and to the same apparent purpose.

(b) All members of the group also attend carefully to breaches of the formulated order. In the event of a breach involving some members, the group never disintegrates, and it is unlikely to do so without a specific closing procedure (Deutsch 1977, Kendon 1977, Schegloff and Sacks 1973). Most often, one of two order-preserving activities takes place on the occurrence of a breach of the formulated order. On the one hand, a group can quickly reorganize back to the context at hand without any acknowledgement that anything was wrong. The context proceeds as usual, the only difference being that the contextualizing 'work load' was temporarily carried by only one or two people, while the rest of the group put the context on hold. On the other hand, the breach can be attended to by someone, and the group is actively called back into a positioning. In both cases, the order or structure of the context at hand is behaviorally oriented to by the participants. In the latter case, the context also is used as a device for holding members accountable to certain ways of proceeding, and this will be considered separately as the next criterion. Both responses to breach behaviors are rampant throughout the film record under analysis.

(4) *Members usually hold each other accountable for proceeding in ways consistent with the context for their concerted activities.* Perhaps the most compelling of the criteria for locating people's answer to the question of *What's happening?* at any given moment is that the members of any group hold each other to behaving in certain contextually appropriate ways. Depending upon the positioning that the group members achieve, there are only certain kinds of behavior which are acceptable at certain moments, and the members call upon those who misbehave to change their behavior to fit the dominant version of what that positioning should look like. This is most clearly the case in the classroom reading lesson, during which stragglers can be called back into a focus on reading activities by the teacher calling them back, by other members of the group simply stopping their own participation until the stragglers return, or, in a case to be described in detail, by an elaborate postural-kinesic dance in which the children and the teacher quietly round each other up until they all return to the book to read.

This criterion is most intriguing, for in holding each other accountable members often must act in ways which fit the other three cirteria we are using to evalute the ethnographic adequacy of a description of a unit of behavior. In order to call each other back to a particular way of behavior, the chastising members must orient to the break in the order of their behavior, formulate what has to be done, and reestablish their bodies into a positioning that signals the formulated order. And, no less importantly, the people held accountable most often respond by immedi-

ately performing behavior formulated as appropriate for the context at hand. Thus the order in their behavior becomes obvious not only in the ways that they try to hold each other accountable, but in the ways they are held accountable.

For example, in a classroom reading lesson, when the teacher says, *Ted, you're not looking at your words,* she clearly formulates what everyone should be up to at that time; she describes the behavioral markers of the reading positioning, orients to someone not doing that positioning, and finally holds the child accountable. In responding to the teacher, Ted is no less demonstrative of the working consensus which is guiding their interaction; on such an occasion, Ted simply moves back to his book and acts in accord with the other members of the reading group. Thus, both Ted and the teacher offer a warrant for considering the reading positioning to be a significant organizational device and the most immediate answer to any question about what it is that they are doing together.

By formulating an order in their behavior, giving it form with their bodies as well as content with specific actions, by orienting to highpoints in the order and holding each other accountable to it, people manage to organize environments for each other in terms of which they can know for sure what it is that they are doing with each other; together, they manage to do the social order. Because this is how face-to-face behavior is organized, we have a great resource for judging the adequacy of our descriptions of concerted behavior. When people identify the contexts for their behavior, they exhibit how they have organized it. This behavior is available and accessible also to the ethnographer. The following sections are a brief demonstration of the ethnographic method we have been describing.

II. THE STRUCTURE OF SOME CONTEXTS FOR ACTIVITIES

In Table I, we offer a description of the structure of the behavior of six children and their teacher during a reading *lesson.* The children and the teacher spend about 30 minutes at the reading table in the front of the classroom, while the rest of the class works on other matters. The time at the reading table is broken into four rather distinct kinds of activities, or *rounds,* the third of which consists of 11 minutes in which the children are asked to read from their book one at a time with the help of their teacher. This round is broken into distinct *turns* to read by individual children, and occasionally by all the children in chorus.

Each turn can be further subdivided into various *positionings,* or postural configurations, which all the children and the teacher assume in unison and from which they appear to engage in different kinds of interactional and pedagogical activities. At this level, the analysis becomes complex, and considerable space will have to be devoted in the following pages to the notion of a positioning as a minimal level of concerted activity and the most immediate and embodied answer members have to the *What's happening?* queries they put to themselves. During the 11 minutes, they manage to arrange four distinct positionings into a sequence of 24 positionings. So we are talking about some fast acting social organiza-

TABLE I. A Hierarchy of Contexts for the Children and the Teacher While Reading the Book.

School	Time in the classroom (5 hours)																							
Lesson	Time at the reading table (30 minutes)																							
Instructional rounds	Reading the book by turns (11 minutes)																							
Turns to read by person(s) designated	Anna		Maria		Ted				Perry		Maria		All				Perry				Ted		Anna	
Group positionings during each reading turn	II	I	II	I	II	I	II	IV	II	I	II	I	II	I	III	IV	II	I	II	IV	II	I	II	I
Actions	Individual movements which by way of form and apparent consequence are recognizable parts of the group's concerted activities: smiles, gazes, gestures, offerings, complaints, readings, fights, etc. Ideally, the effort of a context analysis is to account for all the moves of individual actors in organizational terms. This chart offers a considerable simplification of our context analysis, in that it allows the reader to assume that every action by every member is in fact constitutive of the group Positionings. Although there is truth in this simplification, we must note that other levels of concerted activity could be written into the chart between Positionings and Actions, particularly the many two, three or four person coalitions which develop within most Positionings. A thorough account of these and how they work in the production of more inclusive levels of behavior listed in the chart would greatly enhance the adequacy of our ethnographic description.																							

tions. The members use one of these (Positioning I, which occurs nine times) when they are actively involved in reading or some related activities. The other three are organized for pedagogically more peripheral tasks such as getting organized for a turn to read (Positioning II, which occurs 11 times), waiting for the teacher (Positioning IV, which occur 3 times), or behaving anarchically (Positioning III, which occurs one time). Idealized examples of Positionings I, II and IV are offered in the line drawings in Figures 1–3.

Lower level units of analysis than positionings are the various *actions* of the different group members. Most of this behavior is performed in the service of the more inclusive units in the hierarchy of behavior listed in Table I. That is, most individual actions (statements, arguments, hand raisings, postural shifts, and the like) can be understood as constitutive elements of the positionings, which are in turn constitutive elements of the turn, the lesson, and the time at the table. We were surprised to find that so much of the individual member's activities were constitutive of the activities of the group. After extended observation, however, we saw that almost every movement, every wave of a hand or turn of a head, was well-timed and spatially organized in terms of the concerted activities of the group as a whole. It became clear that almost every movement, at the time of its occurrence, helps to form the contexts which the members use to construct and maintain the units of concerted activity which give order to their next concerted activities and to our analysis.

Each box in Table I represents a unit of the group's activities. Together in taxonomic form, the units represent the structure of the members' activities during their time at the reading table. In order for such a de-

FIGURE 1. Positioning Ia: Reading the book.

FIGURE 2. Positioning II: Getting-a-turn to read.

scription to be ethnographically adequate, the units described must be shown to be the products of the structuring activities of the members trying to make sense of each other (Garfinkel and Sacks 1970, Mehan and Wood 1975). That is, it must be shown that in the course of doing what they are doing together (i.e., reading, getting a turn, behaving anarchically, or waiting) the members of the group constantly identify for each other what they are doing, and in so doing reflexively structure

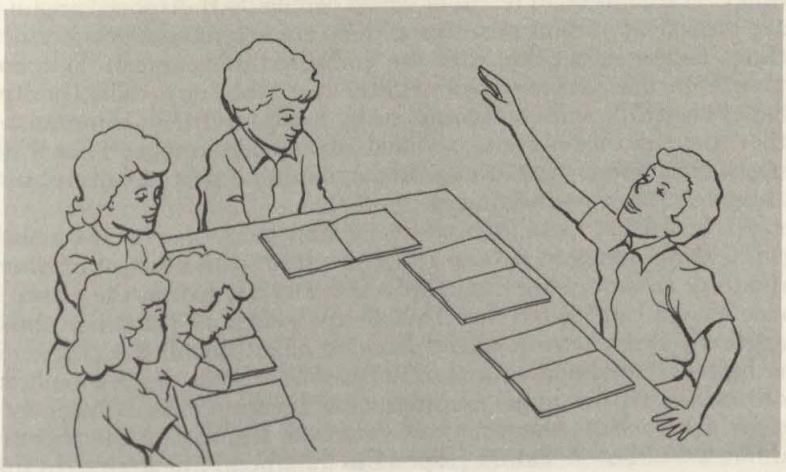

FIGURE 3. Positioning IV: Waiting for the teacher.

their activities in ways which show up on the chart as units of the structure of their concerted behavior.

Although we have listed five levels of context in terms of which most of the members' activities can be understood, there are no doubt many other contexts according to which members might be organizing their behavior. Some of these contexts might be individual (fantasies, for example), and others quite pervasive and systematic (for example, the members' participation in American culture and the political economy which supports that participation). We will make no attempt here to discuss such contexts, although in other papers we have argued that fine grained analyses of small samples of behavior can provide considerable insight into cultural and institutional order (McDermott and Roth 1978, McDermott and Gospodinoff 1978). In this paper we will limit ourselves to examples of a single level of behavior, that of the positioning. We are doing this not only because the main point of the paper is methodological, but because this particular level of behavior is central to how people determine "for sure" what it is they are doing with each other at any given time. Before applying our criteria to the description of the structure of behavior at the level of the positioning in the reading lesson, we need a more extensive description of what a positioning is and why it may be an important unit of description and analysis to the ethnographer.

III. POSITIONINGS AS CONTEXTS FOR ACTIVITIES

Our problem is to describe the contexts or environments people use to limit each other to performing the particulars of whatever they are doing together.

Fortunately, students of body motion and communication have been working on this problem and have given us a record of how the postural configurations people achieve with each other function as the most immediate environments for the organization of their behavior with one another. The contexts in terms of which people do their social organizations are embodied in their postures as these are arranged across persons over time. To the extent that they are embodied, we can call them environments. In the communicational literature, they are called positions by Scheflen (1973) and configurations by Kendon (1973). In order to stress that they are ongoing interactional accomplishments and not fixed entities, we call them *positionings*, in the sense that participants in any group consistently are positioning each other.

A topological metaphor offers a useful gloss of what we mean by a positioning. Imagine people to be involved with each other through a medium of elastic sheeting (Birdwhistell 1970). Just as the elastic pulled at one end can stretch and reshape the elastic at the other end, every move by every person would have an effect on all the others. So it is in human interaction, although the media through which members work on each other are more complex. Now imagine that, before any single part of the elastic sheeting was stretched, the rest of the sheeting was informed of how it should respond to the upcoming stretch. In this way, all the parts of the elastic could move at a single time to a new organiza-

tion. So it is in human interaction. Not only do people adjust themselves to each other, they often do it at the same time. The important thing about the notion of positioning is that people not only organize themselves posturally in relation to what they are doing together, but that they take on the postures characteristic to what they are doing together at exactly the same moment. As such, they appear to be responding to some higher order contexts or working consensuses about what they are doing together. In moving in and out of various positionings together (as in moving through Figures 1–3), people constitute contexts or environments for their next moment, offer information about the consensual reality of their present moment, and supply any analyst with the material for describing what they are doing together.

In the 24 positionings in Table I, most are established and broken in concert. In Table II, we have listed just how carefully the members of the group participate in the beginnings and endings of the nine occurrences of the reading positioning (I). This is an extraordinary record of fine tuning by the members of the reading group. Consider the work they must do to arrange the consensuses which allow all the members of the group to move approximately at the same time, in the same way, and to the same apparent interactional purpose. Both the sequences and consequences of members' activities appear to be well organized at the level of the positioning, and the work that members do to arrange these positionings offers us a useful tool for describing the contexts for the organization of concerted activities.

Although the positioning is a useful descriptive tool, we must remember that a postural configuration, by itself, is never an exact calculus for anyone wondering just what the members of the group are doing at a particular time. Each positioning, on each occasion of its occurrence, is a delicate balance of members knowing approximately what to do, doing it, carefully monitoring how everyone else is doing it, and holding each other as close as possible to the working consensus achieved by the simultaneous attention to the task at hand. The attentional and postural balance is always dynamic. Members constantly move in and out of a postural configuration shared by others in their group; sometimes they are called in or pushed out. In this way, members are constantly and dynamically identifying for each other and for any observer just what they are doing together.

Of the four positionings listed in sequence in Table I, we will concentrate on the analysis of positioning I during which the members are organizing themselves to read or discuss their reading books. In Table III, we have listed some behavioral features of the reading positioning. The following scenario is expected and often appears. When the teacher calls on a child to read, all the children move down to their books at approximately the same time. The child called on begins to read, and the others listen or read along in chorus. Ideally, everyone remains in this configuration until the designated child has finished reading, and the rest of the children look to the teacher to find out who is to read next. Between these clearly marked beginnings and endings, during a positioning I, the members concentrate their attention on the book and use the desig-

TABLE II. Reading Group Members Behaviorally Orienting to the Beginnings (B) and Endings (E) of the Reading Positionings.

	Turn 1		Turn 2		Turn 3		Turn 4		Turn 5		Turn 6		Turn 7		Turn 8		Turn 9	
	B	E	B	E	B	E	B	E	B	E	B	E	B	E	B	E	B	E
Anna	x	x	x	x	x	x	x	x	x	3	x	4	x	x	x	x	x	x
Ted	x	0	x	0	x	x	x	x	x	3	x	4	x	0	x	x	x	0
Perry	x	x	x	x	1	2	x	x	0	3	x	4	x	x	x	x	x	x
Timmy	x	x	x	x	1	2	Off-camera for the rest of the lesson.											
Rosa	x	x	x	x	x	2	x	x	x	3	x	4	5	x	x	x	x	x
Maria	x	x	x	x	x	2	x	x	x	x	x	4	x	x	x	x	x	x
Teacher	x	x	x	x	1	2	x	x	x	3	x	4	x	x	x	x	x	x

Legend:

x. orients to beginning or ending of the reading positioning.

0. maintains a reading positioning throughout the change of turns.

1. Timmy sulks, Perry watches Timmy, and the teacher attends to their breach until Perry returns to the expected position.

2. As Ted reads, another teacher enters the room. All but Ted and Anna attend to the visitor, and Ted's turn never clearly ends.

3. As the turn ends, the teacher is attending out of the group, and the children spread their attentional focus about the room. The teacher returns in time for Maria to complete her turn, and the rest of the children slowly catch up (7 seconds) and recalibrate into a Positioning II calling for a turn to read.

4. The teacher exits to remove a visitor from the reading table while the rest of the children read in chorus. Gradually, the children stop reading and attend to the teacher's struggle. Then they simultaneously move into a positioning III, or anarchy, and the teacher rushes back to the table.

5. Rosa squirms about, a lone example of a breach which is not intelligible in social organization terms.

Explanation:

A table of regularities in the member's orientations to the beginnings and endings of Positionings does not constitute, in and of itself, an analysis. An analysis exists only when this behavior is contextualized. Accordingly, exceptions to the regularities are not significant challenges to understanding the members' behavior. What is of primary interest is how these exceptions are accomplished, how they are treated as exceptions, and how they are repaired:

1, 3 and 4 are not exceptions which prove the rule statistically. They are structurally incongruent behaviors for which members are held accountable.

2 is an exception from outside the system.

5 is not something for which we can account yet.

TABLE III. The Behavioral Features of the Reading Positioning (I).

Features	Positioning I	Variants of Positioning I
Shared focus: the degree to which the members of the group direct their head-eye orientation either to the same or similar objects	Single focus by each member to the reading book	Multiple focus to the book, the teacher and the board in response to the teacher's questions
Concerted activities: the degree to which members move parts of their bodies in time with each other in apparent response to some identifiable contexts	Fairly constant between beginnings and endings	
Vigilance: the degree to which each member must actively monitor other members, particularly the teacher, in order to receive some explicit instruction as to what to do next	Minimal, as the members follow the reading of the book	More necessary during question-answer sequences
Carpentering: the degree to which members assume postures which fit the contours of the table around which they sit	Tight fit around the table, with the corners marked by elbows and uniform shoulder/table angles	Opens slightly to allow for members to see the board when that is necessary
External border work: the degree to which members actively mark themselves off from the rest of the class by way of gaze aversion, the careful placement of appendages at possible entry points to the reading group, and even by occasional confrontation	Intense, as outsiders are ignored and excluded	
Internal border work: the degree to which members compete with each other for access to some resources, usually the teacher's attention or favor, by blocking each other from the teacher's view or attacking each other	Minimal	
Talk: the degree to which members consistently produce particular kinds of speech acts	Pedagogical talk, mostly reading, choral and individual with others following along in their books	Content questions and instructions from the teacher, word calling and answers from the children

nated reader as a drum major. Thus, they move together in congruent postures which match the form of the table; they appear oblivious to outsiders and ignore each other except during breaches to the expected order. A checklist of idealized behavioral features is not, however, a sufficient account of what the members are doing with each other. Until it is possible to understand the members' behavior in the same way that they do, whether they can articulate that understanding or not, it will not be possible to present an adequate ethnographic description. To this end, we will ply our criteria to the work the reading group members do in positioning each other.

IV. APPLYING THE CRITERIA: DESCRIBING ORDER IN APPARENTLY CHAOTIC BEHAVIOR

Table I offers a fairly adequate description of behavior at the reading table, in the sense that every unit in the description can be said to be formulated and positioned by the participants and that the maintenance of the formulated order is oriented to and used by the members to hold each other accountable. Every unit in the taxonomy is acted on by the members.

The description of these units was quite difficult, particularly at the level of the positioning, for it is often the case that not everyone produces idealized postures and behavior and that a particular positioning will be temporarily invisible. It appears that members are attending to some other activity than the reading lesson. At such times, members orient to their breach of the formulated order and hold each other accountable for returning to that order. In addition, even though the members sometimes seem to be attending elsewhere, they maintain positioning I as a context and a reference point for that wandering. Thus even when they are not acting as if they were in positioning I, it is positioning I and nothing else that they are *not* doing (Frake 1977, McDermott 1976). In order to observe that their apparently chaotic behavior is regularly sequenced and has predictable consequences, it is necessary to describe how the members do their organizing work.

The specific example we will discuss seemed especially chaotic to us and other observers as well. It is a few seconds of behavior which did not seem to make sense in terms of what preceded and followed it. In other words, it appeared to be without a context and apparently random and chaotic behavior. However, when we examined this activity in terms of the four criteria for an ethnographically adequate description of behavior, its structure and sense became evident.

The example we are discussing is Maria's second turn to read. It does not display the behavioral features of a reading positioning. Remember that a reading positioning is marked by everyone looking at their books in congruent postures carpentered to the table, moving in concert, and reading with the assigned reader (see Figure 1 and Table III for a definition of terms). The teacher calls on Maria to read, and soon everyone is looking in different directions. How could this be a reading positioning without being acted out as a reading positioning? The answer is that,

even though they do not act it out immediately, the members of the group formulate a reading positioning when Maria is called on to read, and they are liable to being held accountable if they do not act it out. The demonstration of this is quite subtle, and we will offer it in some detail. The reader is encouraged to follow the figures (4–9) along with the text.

The beginning of Maria's turn is complex. Immediately after the teacher starts to call on Maria, Rosa calls for a turn. The teacher responds to her behavior as a breach in the order and says, *Rosa, we don't call out.* Then the teacher again asks Maria to read. Again the group begins to settle into a reading positioning. But before they complete their shift, the teacher leans out of the group to speak with Audrey who has been sitting on the floor near the reading table. The two children nearest the teacher, Anna and Ted, follow her shift in gaze out of the group. Soon, Perry and Rosa also are looking around the room (Figure 4). So Maria has the turn to read, but she has no audience, and the reading positioning is basically invisible.

At this point, what takes place is an incredibly precise dance involving most of the group members. The dance is made up of five movement clusters, involving four members of the group. Initially the group is dispersed as in Figure 4. At the end of the dance, about two-and-one-half seconds later, the children and the teacher are back in a reading positioning. All this happens without a single word being said.

Assume the group to be positioned as in Figure 4. After dealing with Audrey on the floor, the teacher starts to return to focus on Maria who has been reading the assigned page. On the same frame (at 24 frames/second) of the film in which the teacher completes her return to Maria

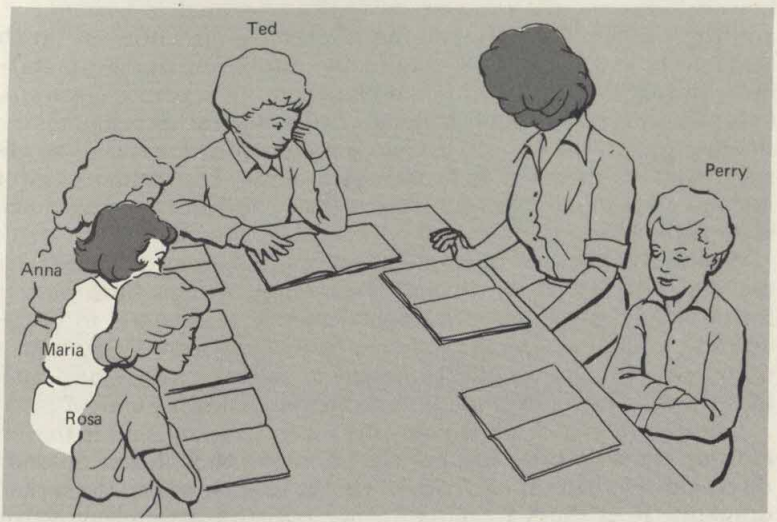

FIGURE 4. Reading Positioning I Turn V: Maria reads without an audience.

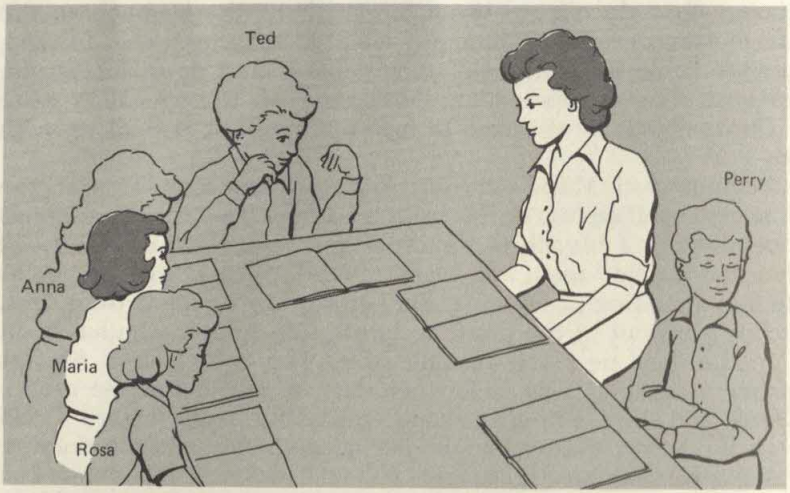

FIGURE 5. Reading Positioning I Turn V: Teacher returns to the group, and Maria stops reading.

and the group, Maria stops reading (in the middle of the assigned text) and starts to move her head up from reading. And within the next five frames, Anna and Maria have started to move their books down to the table, and the teacher orients to them by jerking her head back and up, resulting in the scene depicted in Figure 5.

The second cluster of movements begins 12 frames (one half second) later. Rosa and Maria move their bodies down to the table, a move they will build on in the next two clusters of movements. For the time being, however, the stunning movements are performed in concert by Perry and the teacher. In Figure 5, the teacher has just oriented to the fact that Maria has stopped reading in the middle of her turn, and it can be seen that Perry is looking out elsewhere. It becomes apparent, however, that Perry has kept a careful feeler out for developments in the reading group. On exactly the same frame, both Perry and the teacher start to move their heads toward each other. The culmination of their actions, with Perry turned to the teacher, and eyes gazing downward, is depicted in Figure 6.

The third cluster of movements occurs 12 frames after Perry and the teacher started to move towards each other. At this time, on the same frame, Maria stops her movement forward, Rosa starts to move back, and the teacher begins to move her head away from Perry to her right. Only two frames later, Perry begins to collapse his posture into more of a slump. These developments should be clear in Figure 7.

The fourth cluster of movements starts 12 frames after the previous cluster. Maria and the teacher start to move their heads up and in the direction of looking at each other. On the same frame as these two developments, Rosa starts to move to her right, down and away from the

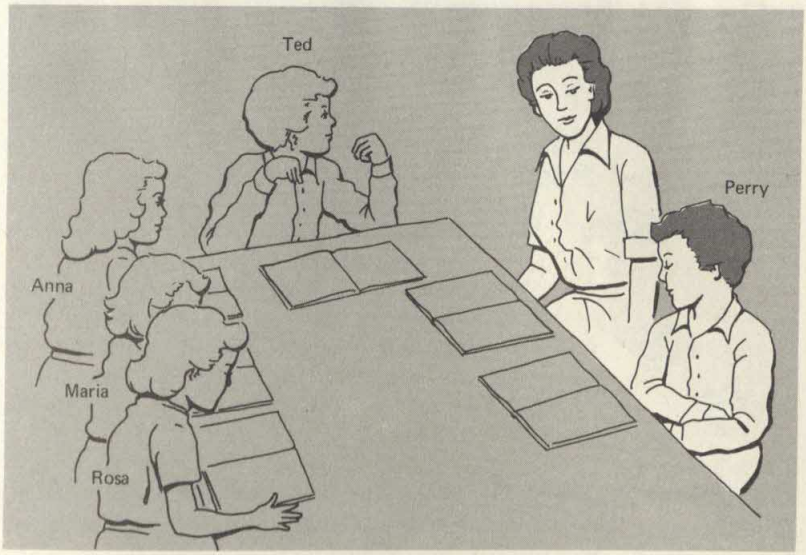

FIGURE 6. Reading Positioning I TURN V: The teacher and Perry turn to each other as Maria and Rosa move into the table.

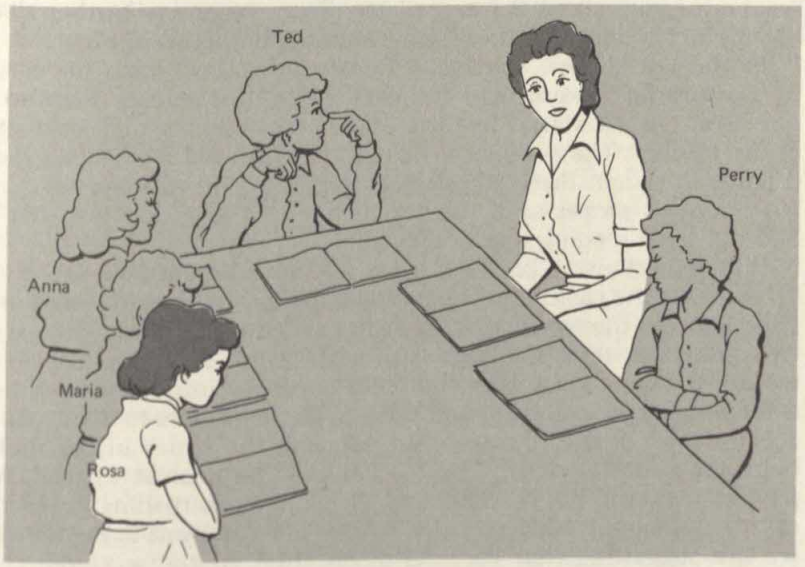

FIGURE 7. Reading Positioning I Turn V: The teacher starts to move her head to the right, Maria stops moving, and Rosa starts to move back and away from the teacher's passing glance.

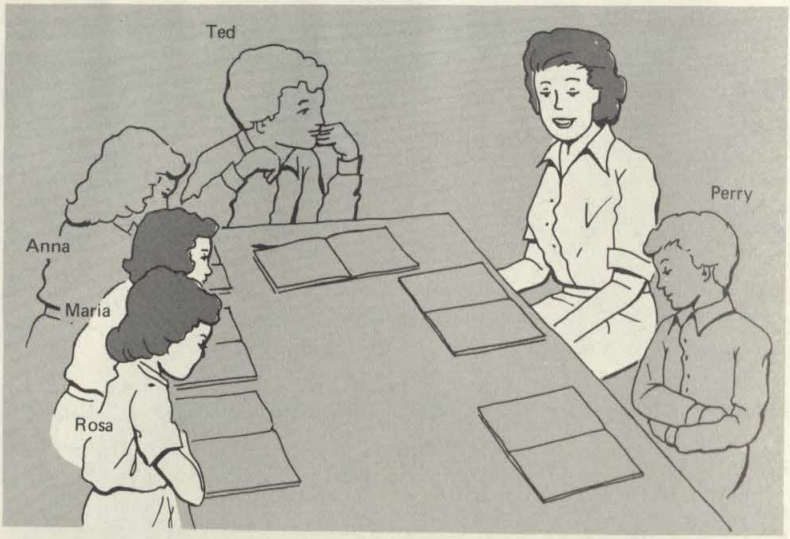

FIGURE 8. Reading Positioning Ia Turn V: The teacher and Maria start to move their heads up. Rosa continues to make herself both unavailable for reading but part of the group as she moves into a reading positioning, but down and away from the teacher.

teacher. Again, about 2 frames later, Perry begins to further slump in his chair. The result of these movements is depicted in Figure 8.

In the last cluster, starting 8 frames later, Ted starts to reorganize his posture for reading. On the next frame, the teacher starts to move her head toward Maria. In turn, Maria stops moving and looks intently at the teacher. The product is a group recalibrated for reading the book (Figure 9). When they are all back together, the teacher says only, *A little louder, please,* as if the problem with the group was that Maria had not been reading loudly enough.

It is important to be clear what all this is about. We are trying to substantiate the claim that the positioning units or contexts listed in Table I are actually the contexts attended to and used by the members of the two groups in their dealings with each other. In order to prove this, we are trying to show that the order apparent in different positionings is formulated, oriented to, and used by the members to hold each other accountable. In the example just detailed, the order in the members' behavior is not obvious. The teacher calls on a child to read, no one pays attention to the reading, and the child stops reading in the middle of the assignment. Although the order is not apparent to the uninitiated, we are suggesting that there is much order in the ways the children and the teacher deal with one another. Although the attentional structure of the group seems to spread around the classroom, in this case the members of the group keep a careful watch on each other. As it is a reading positioning for which they have formulated and organized themselves,

FIGURE 9. Reading Positioning Ia Turn V: The teacher and Maria move into the book, and the rest of the group organizes for the second start of the reading positioning for Maria's turn to read.

the members are quite sensitive to certain kinds of movements. During a waiting positioning (IV) Maria could put her book down, and no one would notice, or the teacher could turn to Perry, and he would not attend. But in the reading positioning (I), because the members understand what it is that they are doing with each other, even if they are not doing it in its idealized version, the fact that Maria stops reading or that the teacher turns to Perry takes on meanings to which all respond, meanings to which they all hold each other accountable. These interpretative grids frame or contextualize the members *vis-à-vis* each other.

In the members' behavior, in this example, we have a display of how contextualizing work is done and how the contexts for activities are used to regenerate a particular social order. Maria stops reading and gets the teacher's attention. The teacher brings Perry back into the group. Rosa moves with the teacher, as she is wont to do, in a way to keep her as part of the group, but unavailable for reading or mutual gaze with the teacher. All this brings Ted back into the reading positioning. With the group reorganized in a reading positioning, Maria can once again start to read.

Interestingly enough, in less than a minute from this sequence, at the end of Maria's turn to read, the group members go through an almost exact replica of this sequence of behavior and achieve the same result, that is, everyone comes back to the reading positioning after an elaborate recalibration dance. These examples should show how the members actually understand a great deal about what it is they are doing with each other and how they use these understandings to contextualize their communication with each other. It should also make clear how we can use

the understandings they exhibit *in situ* about their contexts to judge the adequacy of our own descriptions of their behavior and the organizational contexts in which it occurs.

CONCLUSION

People achieve order in face-to-face behavior by formulating a working consensus of what they are doing with each other, by orienting to and holding each other accountable for the proper ordering of their behavior, and by doing it in such a way as to allow for the sequential proposal and possible confirmation of their consensuses. Ethnographies which do not take into account the interactional complexity of the order achieved in face-to-face behavior is likely to have an inadequate descriptive foundation for making replicable and generalizable statements about the organization of behavior in different groups. To remedy this situation, we have suggested that the ways people have of managing their own sense of what it is that they are doing with each other should be used to judge the adequacy of any description of behavior and its contexts. Thus, we have suggested that it is necessary to determine the adequacy of any description of the form and content of concerted behavior in terms of whether it is (1) formulated, (2) posturally positioned, (3) oriented to, and (4) used to hold members accountable for certain ways of proceeding. In the development of an example to illustrate these criteria, we have claimed to demonstrate ordered behavior where others saw chaos.

NOTES

1. Ethnographers often call for descriptions which could be evaluated by readers according to a set of criteria of the type we are suggesting, but every year we are offered a new set of statements of apparent defeat to the effect that nothing can replace the personal intuition and insight of the ethnographer. We are not trying to replace the good sense of the fieldworker. Adequate description depends heavily on the practical knowledge and deep feelings of the ethnographer. But in celebrating the intuitive flash of the fieldworker, we must not lose sight of descriptive clarity and rigor as goals. We simply are asking for some standards for judging the behavioral accounts which must form the basis of any ethnographer's display of sensitivity and insight. Most ethnographers share our concern, although Geertz's (1973) influential call for "thick description" and thin criteria for the evaluation of a description has sparked a number of claims that ethnographers can only assert and never display enough data to make replicable descriptions consistent with the natives' interpretations of their own activities. This position can force ethnographers to embrace good literature as the only goal for their descriptive efforts. We differ to the extent that we want to encourage both literary and more replicable approaches to ethnographic description, and we offer the following criteria as one way of distinguishing the two kinds of efforts.
2. The term *working consensus* originally was used by Goffman (1959:9–10) to refer to a "kind of interactional *modus vivendi*. Together the participants contribute to a single overall definition of the situation which involves not so much a real agreement as to what exists but rather a real agreement as to whose claims concerning what issues will be temporarily honored." The term has been used by Kendon (1976:322) in much the same way we have

been using it: "A central problem in the investigation of interaction . . . will be to see how, in terms of the functioning of observable behavior, the 'working consensus' for a given behavioral system is established and maintained. In particular, this means that we must identify those aspects of behavioral function which serve to control or regulate the behavior of the participants in relation to the currently established pattern of relationship. This requires that we look for regularities in behavioral relationship, but also that we look closely at places where these regularities change."

Although Kendon uses the term to refer to behavioral regularities and functions, the fact that thinking is involved in the process of forming agreement and consensus suggests that a more cognitive reading of the term is possible. We will play off both senses of the term, although it is important to point out that we are never referring to what might be going on in any one person's head. A working consensus is public, and the term refers only to the minimally precise definition of what is going on that members need to continue in interaction with each other. It is possible, for example, for two people to walk away from the same interaction with two quite different accounts of what they did together. For example, one participant in a transaction may be courting, while the other was unaware of this. At another level, however, they had to have shared some consensus of what they were doing together, in order to carry on their conversation. A working consensus is publicly cognitive in the sense that people can be taken to task for displaying that they are not thinking that they are doing a particular thing together (Frake 1977).

As an expository device, we are going to use a number of terms to refer to different aspects of the contexts people use in their organization of concerted behavior: environment, working consensus, and formulated order. Where it is necessary to do so, we will attempt a rigorous discussion of these terms. For the most part, they should be read as they would be in everyday English, their function being to highlight the different aspects of context which we hope to emphasize, but very loosely, at any point in our discussion. We use environment to suggest the more physical aspects of the contexts people use to organize their behavior, working consensus to suggest the more cognitive aspects, and the formulated order to suggest the more normatively guided aspects of contexts.

3. We will sometimes refer to the problem of delineating what is going on in terms of a question people might ask of themselves and each other, namely, *What is it that is going on here?* (Frake 1977, Goffman 1974). This is not to suggest that we will limit our description to well-framed questions and answers, although we find that the systematic study of *What's going on?* question-answer frames has given us some of the most ethnographically adequate descriptions of activities and their contexts. Cognitive anthropologists interested in events (Agar 1975, Frake 1964, 1969, 1976, Metzger and Williams 1963a, b, Spradley 1970) have worried about the descriptive adequacy of people's answers to such questions, and, on the assumption that the proper questions would elicit the proper answers, they have tried to control for the relevance of people's answers to such queries by the careful use of eliciting frames. In this way, they have offered some excellent accounts of the terms people have available for talking about what it is that can go on in their lives. However, a taxonomy of terms people use to describe their participation in everyday life does not adequately describe what it is that they are doing at any given time or how it is that whatever they are doing comes to be agreed upon and named. Adequate framing for a *What's going on here?* question is considerably larger than any elicited frame can possibly be (Bateson 1972, Wootton 1975). As Frake (1977) has pointed out, the methodological difficulties of cognitive an-

thropology "have arisen from a failure to fully exploit the interactive aspect of the frame model, to widen the frame so as to capture a context that more fully specifies how human behavior comes to have meaning."

This paper uses and develops the interactional aspects of the frame model. One unfortunate aspect of many of the frame analyses of most anthropologists is evident in the cognitive language used to describe the interaction between two or more people; the problems natives had to solve with each other were problems of cognition, of not knowing enough, of not having enough of a mutual understanding. The language of the more interactional approach called for here is, to borrow Bentley's (1954) insightful term, more *muscular*. In addition to sharing knowledge about each other, and whatever it is they are doing together, actors in this paper will *struggle* to make sense of each other and *do work* to help generate the kinds of recognizable contexts for common sense to be achieved from one moment to the next. As Garfinkel (1963, 1967) has pointed out often, the problem facing people in interaction is never simply one of shared knowledge or overlapping interpretive grids. No matter how much people know in common, they must still work at constructing the environments that their mutual knowledge leads them to expect, and any relaxation of this effort can have disastrous consequences. People never know exactly how to make sense of each other. Rather they must do interactional work to create the kinds of environments which members can recognize as suitable environments for displaying whatever it is that they know how to do with each other.

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19

Talking About Talking About Residence: An Akamba Case[1]

STEPHEN M. FJELLMAN*†

There are a number of ways to describe residence patterns. The traditional approach begins by coding the people of a sample in terms of categories given by general ethnological theory (neolocal, patrilocal, etc.). The numbers in each category are totaled, and various simple statistics (mean, mode, median) are drawn from the data. A descriptive statement about residence is then made, and the ethnographer is able to use this residence pattern in formulating his hypotheses about social organization.

A second method, associated especially with the names of Fortes and Stenning, adds change over time to the description. People are assigned to categories of residence so that each household may be typed in terms of composition. Then statements can be made about the domestic cycle and patterns of change in household composition and residence. This second approach has certain obvious advantages over the mere enumeration of the first method. Knowing, for example, that household type B generally develops from household type A through some change in membership, one can attempt to isolate the structural or behavioral strains implicit in the latter and the ways in which a change to type B may resolve these strains.

Both of these approaches make use of residence categories drawn from general ethnological theory. They account for residence as seen by someone outside the social group under consideration. They are not complete, however, because they tell us nothing about how the individuals of that group understand the system they (literally) live in.

The description presented here differs from those mentioned above in that the categories used are elicited from the people under study themselves. It was inspired by Geoghegan's path-breaking work on decision making and residence among the Semal of Tagtabon Island in the southern Philippines (Geoghegan 1971, n.d.). Using information about personal status, Geoghegan constructs a set of ordered decision criteria which predict, with a very high degree of accuracy, mode of residence among a group of 285 Semal. The emphasis in this study differs from Geoghegan's in its explicit focus on a technique for eliciting the relevant ethnographic information on personal status and residence categories.

People of a particular community have ways of describing how their neighbors reside. "He lives alone . . . She lives with her mother." If someone lives in a bizarre manner, people can tell you why it is bizarre. Moreover, they can usually explain *why* an individual resides in one way or another. This explanation consists of particular facts about that person's

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† Source: *American Ethnologist* 3:671–682, 1976.

status, e.g., "She's divorced and couldn't get a job, so she's back with her parents." If a group has a residence pattern, that should mean that people with the same status characteristics will, for the most part, be living in the same mode. People ought to be able, when told what characteristics an individual of their cultural group has, to place him or her in the right category. The remainder of this paper will consist of a description of the Akamba of the southern portion of Kilaloni Location, Machakos District, and what they talk about when they talk about residence.

The purpose of my model is not to duplicate actual cognitive processes. What it does do is to organize indigenous ideas in such a way that an outsider could predict Kilaloni Akamba residence categories from social status characteristics and could do so in the most efficient manner possible.

THE PEOPLE

The 480 Akamba whose comings and goings are reported in this study live in forty compounds. These form a continuous block of habitation, separated from the northern portion of Kilaloni sub-location by the Kingagi River.

Kilaloni Location, which is only forty-two miles from Nairobi, is reputed to be a very progressive area for Ukambani. Over 90 percent of the children between the ages of eight and twenty have been in school at one time or another. The sub-location has two nursery schools. 75 percent of the compounds have at least one building with a sheet metal roof. None of the girls in this area have attended circumcision ceremonies since 1960.

Although 59 percent of the male household heads earn income from other sources, every compound engages in agriculture, growing crops for both subsistence and market. All families grow coffee and belong to one of two nearby cooperatives. 38 percent of the compounds keep some cattle in the location, but Kilaloni is sufficiently well provided with rainfall that there is very little unplanted land left for grazing. Thus people either keep very few cattle or graze them outside the sub-location.

THE DATA

The basic data for this description of residence came from the census material collected for the Child Development Research Unit. Each of the forty compounds in the area was surveyed during an average of about eight visits apiece. Information was gathered on household composition, present place of residence of all people considered to be household members (some clan and lineage members live and work outside Kilaloni, returning at varying intervals), marital status and history, education, religion, health, residence and employment history, and so forth.

With this information as background, I began talking to my informants about residence. My method was to choose an individual from the sample, and, without revealing the identity of that person, ask my informants to decide how that person was residing by asking me questions about him. I instructed my informants that I would only answer questions "yes"

or "no" and that they could ask no direct questions about residence. When they had made a decision about that person, I would pick another and repeat the process until I had covered what seemed to me to be a fair range of residential circumstances.[2]

These question and answer sessions gave me three bodies of information: (1) a list of descriptive phrases in Kikamba about residence conditions; (2) a series of questions about personal status characteristics; (3) an ordering of those questions such that my informants could use the answers efficiently to assign the individual in question to a residence category.

The residence categories are as follows:

Kikamba Phrase	English Gloss
1. men	
1A. *ekalaa na asyai make*	'stays with his parents'
1B. *ekalaa na nyinyia*	'stays with mother'
This phrase is used when Ego's mother is widowed or divorced, or when Ego is an illegitimate son. The connective *na* ('with') implies that the mother is supporting the son and is used with reference to unmarried males. If the son were married, according to my informants, he would be supporting his mother and the *na* would be replaced by *kwa* ('at') to convey the correct implication (cf. 1F).	
1C. *ekalaa* + place name	'stays (or lives) at' + place name
This phrase describes the people who are considered full members of the households in the sample, but who are staying elsewhere for one reason or another, returning home when free. It is not necessary to specify what males are doing if they live outside the sample, although the following phrases are used for greater precision:	
athũkũmaa . . .	'works at . . .'
asomeaa . . .	'is schooling at . . .'
If it is known that an individual stays with a relative, this phrase will be included.	
1D. *ekalaa na aka make*	'stays with his wives'
1E. *ekalaa na ana make*	'stays with his sons'
The implication here is that at least one of the sons is married. If only one of these sons is married, it is expected that it will be the youngest married son, who traditionally remains behind when his older brothers move out of the compound. In this case the correct phrase is *ekalaa na mwanae,* 'stays with his last son.'	
1F. *ekalaa na mũka kwa nyinyia*	'stays with wife at [his] mother's'
Here the *kwa* suggests that the son is supporting his mother after the death of his father (cf. 1B).	
1G. *ekalaa kwa asyai make*	'stays at [his] parents' place'
The *kwa* here implies that the son is married. It is opposed to the *na* in 1B, which is used for an unmarried son.	
1H. *ekalaa na muka*	'stays with his wife'

Kikamba Phrase	English Gloss
This phrase implies the presence of unmarried children, although *na syana syake* ('and his children') may be added for precision.	
1I. *ekalaa na mwana wake*	'lives with his son'
This phrase means that a man lives with only one of his sons, and not necessarily the youngest married one.	
1J. *ekalaa kwake, nĩ ngũnzi*	'he lives at his own place, is a man without a wife'
or *ekalaa kwake, nĩ-walũngilye kiveti kyake*	'he lives at his own place, he chased away his wife'
2. women	
2A. *ekalaa na asyai make*	'stays with her parents'
2B. *ekalaa na nyinyia*	'stays with mother'
See 1B.	
2C. *ekalaa* + place name followed by a descriptive phrase stating what the female is doing	'stays at . . .' + phrase
As in the case of males, this phrase is applied to a girl or woman who is considered a full member of the household, but who is presently living elsewhere. In order to suggest that the female in question is behaving in the proper manner, it is necessary to specify what she is ostensibly doing away from home.	

Each of the next four categories are split in two. A married woman's domestic situation differs for any category of residence depending on whether or not her husband lives at home. This distinction is made by adding the phrase *ĩndĩ mũũme athũkũmaa* + place name ('but [her] husband works at . . .') to the residence description that applies to her.

2Di. *ekalaa na mwĩve*	'stays with co-wife'
or *ekalaaa na mwĩve na muume ekalaa o nao*	'stays with co-wife, and husband stays together with them'
2Dii. *ekalaa na mwĩve, ĩndĩ mũũme athũkũmaa . . .*	'stays with co-wife, but husband works at . . .'
2Ei. *ekalaa na mũũme*	'stays with husband'
2Eii. *ekalaa na mũũme ĩndĩ mũũme athũkũmaa . . .*	'stays with husband but husband works at . . .'
2Fi. *ekalaa na mũũme na asyai moo*	'stays with husband and their parents'
2Fii. *ekalaa na mũũme na asyai moo, ĩndĩ mũũme athũkũmaa . . .*	'stays with husband and their parents, but husband works at . . .'
2Gi. *ekalaa na mũũme kwa nyinyia*	'stays with husband and [his] mother'
Again the *kwa* is used, because a married son supports his mother after his father dies.	

2Gii. *ekalaa na mūūme kwa nyinyia ĩndĩ mūūme anthūkūmaa . . .* 'stays with husband and [his] mother, but husband works at . . .'

2H. *ekalaa na anae* 'stays with her sons'

The *na* implies that there is at least one married son and that this son supports his mother.

2I. *nĩ ndĩwa, ekalaa na syana syake* 'she is a widow, she stays with her children'

In this phrase, the use of *syana syake* ('her children') rather than *anae* ('her sons') suggests that these children are not married. However, to a Kilaloni Mukamba, the phrase also implies that the woman had been married to her husband and living in his compound for a long time before his death. Otherwise, she would be placed in category 2J, below. Therefore, she will probably be an old woman, and her children, despite the fact that they are now unmarried, will be supporting her.

2J. *nĩ mwĩnzyoka* '[she] is someone who has returned'

This is said of a woman who was married but has come back to her father's place. Such a woman may have been separated, divorced, or widowed before she had been living with her husband long enough to be completely absorbed into his extended family.

THE QUESTIONS

In order for my informants to assign whichever person I had in mind to his or her proper residence category, they needed certain information. Requiring them to ask me questions that could only be answered with "yes" or "no" served two purposes. It gave me the list of social status characteristics that my informants needed to know in order to be able to describe an individual's residence situation. It also gave me the order in which these characteristics were considered in establishing that person's social identity.

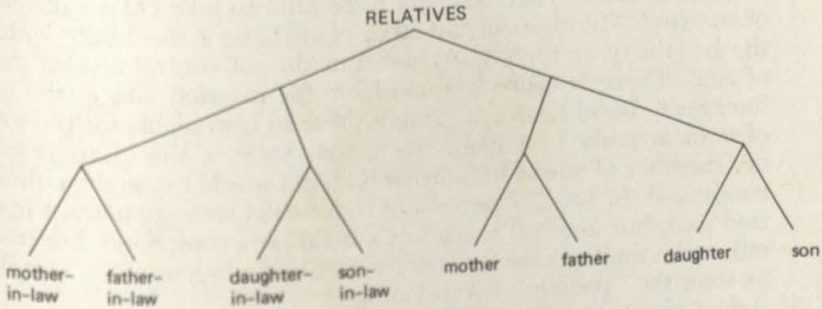
The reasoning behind this procedure is found in information theory. I will explain this reasoning with an analogy, taking the game of Twenty Questions as an example. My contention is that the process involved in playing this game is the same process that we use in gathering information we need in order to make many of our daily decisions, including, possibly, how to assign people to residence categories. Suppose we have to guess what single object in the world another person has in mind in Twenty Questions. If we ask a very specific question, such as, "Is it Charlie's Aunt's nose?" we may win on the first try. If our opponent answers "no," however, all we have done is to eliminate Charlie's Aunt's nose from contention, and we only have nineteen questions left. The most efficient way to play the game is to begin with a very general question, the answer to which will eliminate the greatest number of objects possible from consideration. The best question we could ask would always eliminate exactly half the alternatives, although this is not initially possible in Twenty Questions unless we ask a disjunctive question—a question with "or" in it—such as "Is it animal or vegetable as opposed to mineral?" If the answer is "no," we have eliminated two-thirds of the possibilities,

if "yes," only one third. Once our first question has narrowed down the field, we should ideally try to cut the remaining portion in half with another general question. Thus we keep splitting the field in half until we zero in on the answer. The answers to any question, moreover, limits the type of question that is appropriate to ask next. For instance, if we know that the object to be guessed is vegetable, we should probably not ask, "Does it fly?"

A question which has a yes or no answer is called a binary question, and in information theory, the answer to any binary question provides one "bit" of information about the field of inquiry. For example, given the following paradigm, it takes three binary questions, or three bits of information, to specify any one of the English kin terms on the lowest level[3] (Figure 1).

If everyone's mind worked according to the tenets of information theory, this would obviously be the most efficient of all possible worlds. Although all of the questions used in the model came from my informants, they did not always use the most efficient ordering. Invariably, the first questions were the most general ones (either a question about the individual's sex or his marital status). The questions would become progressively more specific, but often an informant would make a very inefficient choice. If the answer to the question was "no," he did not learn much.

Actual measurement of efficiency is difficult. If I ask one informant to discover how a particular person resides, I can match the number of questions he uses against the fewest number of questions needed to specify that person's residential category. If I ask two informants about the same person, they may need the same number of questions, but may ask different ones, or ask the same ones in a different order. The model I have constructed is based on all my informants' queries about a number of people. It is a composite model, constructed by the ethnographer out of indigenous material. In light of the full model, some sequences of



1. Q. "Is it an affine?" A. "Yes."
2. Q. "Is it in the assending generation?" A. "Yes"
3. Q. "Is it male?" A. "No," thus narrowing it down to mother-in-law.

FIGURE 1. Some American English kin terms.

questions that are very efficient in pursuit of a particular residence category may not be very helpful in getting to others. I do not claim that any inhabitant of Kilaloni holds the entire model in his or her conscious mind. I do, however, claim that he or she is cognizant of at least part of it and would recognize the rest as a valid description of residence in the community. The model does not duplicate actual cognitive processes, but it is based on them.

The questions elicited are for the most part self-explanatory (see decision tree below). The following, however, do need some explanation.

(1) *Is the person in question a youngest married son?* (or with a female in mind, is her husband a youngest married son?) Traditionally, when a group of brothers marry successively, they are free to move out of the family compound and build their own homes nearby, providing that there is enough land. It is the responsibility of the youngest married son, however, to remain behind in the family homestead and take care of his aging parents. Thus if my informants find out that a particular person is a last married son, they will predict that he and his wife live with the son's parents.

(2) *Is the male in question (or is a woman married to) a true member of Muka or Kivati lineages (mbaa)?* This turns out to be a clever way of asking, "Is the man an oldtimer in Kilaloni?" There are eight Akamba clans with male members living in the sample area. Two of these clans, Aĩini and Amũũmonĩ, are represented in twenty-seven out of forty compounds. The others are represented by a few each. Moreover, of the Aĩini compounds, all belong to one lineage, *mbaa* Muka, and all but one Amũũmonĩ compound belongs to *mbaa* Kivati. The remaining Amũũmonĩ household *calls* itself *mbaa* Kivati, but it is well known that Kivati was not one of the household head's direct ancestors. Thus he is not considered a "true" member of the lineage.

Ancestors of what are now *mbaa* Muka and *mbaa* Kivati migrated to Kilaloni many years ago and were able to take charge of most of the land. Members of the other clans have arrived only within the last thirty or forty years and thus do not control a great deal of land. There is more land available for married sons of the big lineages to build their compounds on than is available for the sons of later arrivals. It is likely, then, that most of the married sons of a member of one of the smaller lineages would live in the father's compound. In fact, 77 percent of those men who are not last married sons, but are still living in their father's compound, are from either the smaller clans (which comprise 33 percent of the sample) or from the "pseudo" Kivati family (1).

(3) *Asked about a widow—Was she married to her husband for a long time?* As stated in the explanation of residence category 2I above, a woman who has been married to one man for a long period of time will probably have lost most of her ties with her own family. Thus, if she has children, she is expected to remain in her husband's compound after his death.

THE MODEL

If Kilaloni Akamba with the same social status characteristics do, in fact, reside in the same manner, then the status characteristics suggested by my informants ought to put individuals into the correct indigenous residence categories. To test the theory, I first recorded my data in terms of the residence categories I had elicited. I then made use of a tree structure. As an example of such a model, I will refer back to my simple paradigm of kin terms above (see Figure 2).

Given this set of eight English kin terms, the tree specifies what proportion of the set is made up by each kin term. The tree is read from left to right defining the terms as it goes along. Each diamond shape, or node, is a decision point. At the leftmost node, a question is asked about the entire set in an attempt to split the set in half. The information given by the answer tells which node to proceed to next. The number in parentheses along a path is the proportion of those terms considered in the previous question which take that particular path's value (i.e., yes or no). For example, the first node asks if Ego (the individual in question) is an affine. Suppose Ego is not an affine. The questioner then takes the path marked "no" to the next node. One-half (.500) of the terms in the set are not affinal terms. The next question inquires whether or not Ego is a member of an ascending generation. If not, the path marked "no" is again taken. The proportion here means that .500 of those terms in the set that are *not* affinal terms are also *not* terms for members of an ascending generation. The proportion of terms left, then, after two questions are answered is .250 (.500 × .500). The next question again cuts the number in half (.250 × .500) leaving each defined term as .125 of the set. By multiplying the final proportion at the end state, or rectangular-shaped form (e.g., for son) by the total number of terms in the set (8), we find that the tree predicts that one term will be at

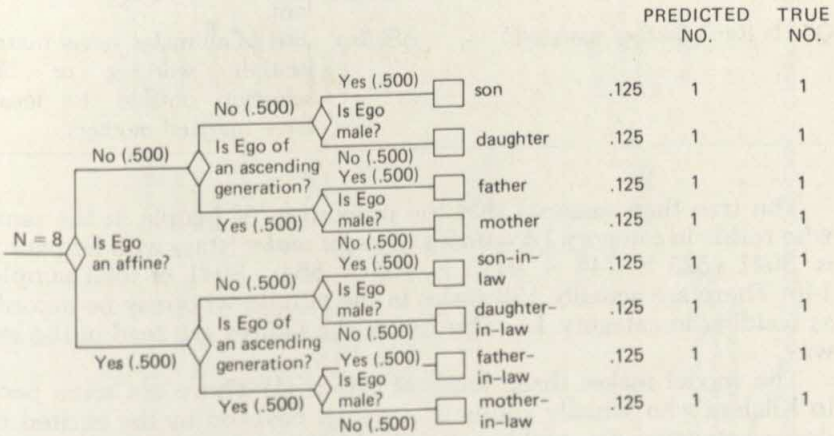


FIGURE 2. Tree diagram of ordered question specifying some American English kin terms.

the end of the path taken. The actual number is one, so the chain of proportions has been accurate for that path. As we move through the tree gathering information, the definitions become progressively more precise and the number of cases that fit the definition at any particular moment becomes smaller and smaller. By the time all the end states have been reached, the definitions and proportions have been allocated as accurately as the ordered questions allow.

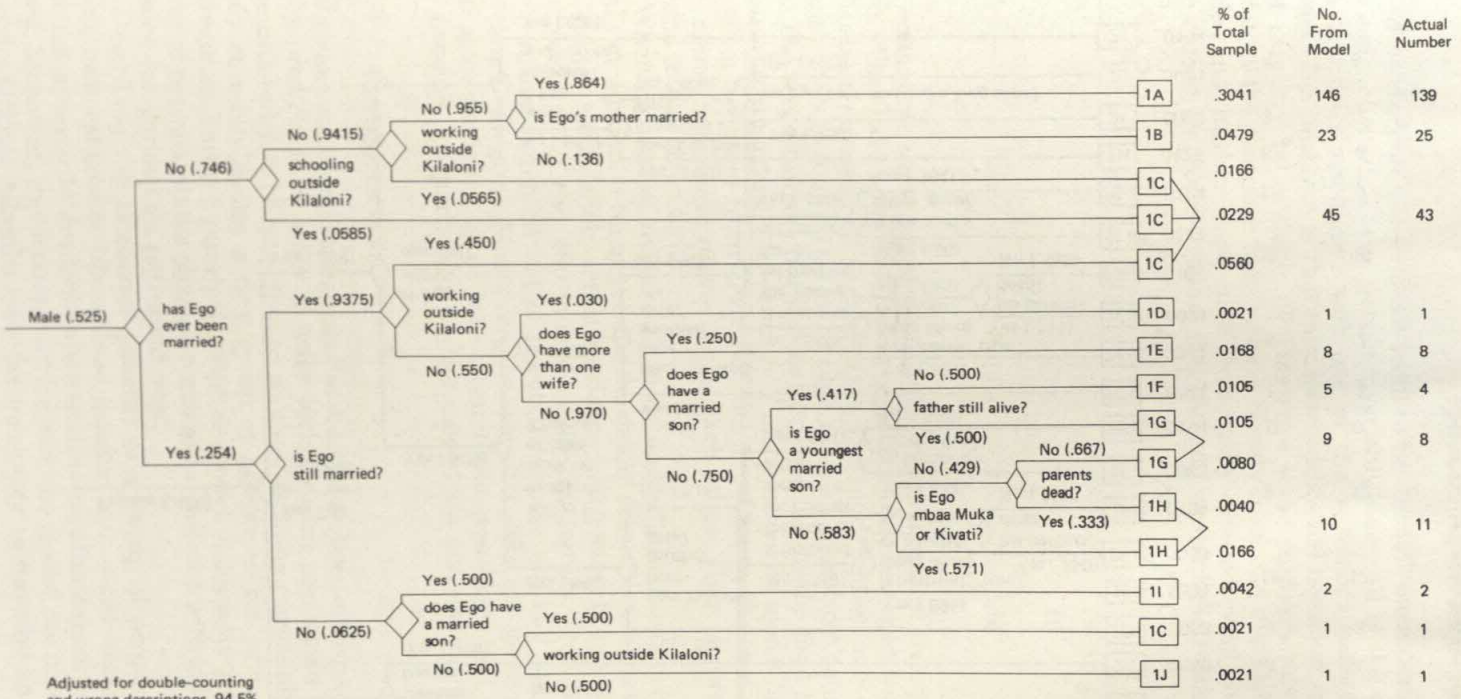
The tree diagrams which follow work according to the same principles. The structure of the semantic domain, however, is a bit different. In the English kin term example, the domain is a paradigm. The distinctive features used to define the domain are independent of one another. Thus the questions may be asked in any order without any loss in information or efficiency. The distinctive features expressed in the questions on the residence tree are not independent of each other. An answer to one question automatically rules out other questions. Many of the questions are path-specific. Thus the probabilities shown in the residence tree are conditional probabilities.

The proportions in the diagrams are taken from my census data. The topmost path on the figure for Males (Figure 3) may be read as follows:

Q1: Is Ego male?	A1: Yes	.525 of the whole sample of 484 is male.
Q2: Has Ego ever been married?	A2: No	.746 of all males have not been married at one time.
Q3: Is Ego schooling outside Kilaloni location?	A3: No	.9415 of all males who have never married are not being schooled outside the location.
Q4: Is Ego working outside Kilaloni?	A4: No	.955 of all males, never married, not being schooled outside the location, do not work outside Kilaloni.
Q5: Is Ego's mother married?	A5: Yes	.864 of all males, never married, neither working or being schooled outside the location have married mothers.

The tree then suggests that the proportion of people in the sample who reside in category 1A (*edalaa na asyai make,* 'stays with his parents') is .3041 (.525 × .746 × .9415 × .955 × .864). .3041 of total sample is 146. There are actually 139 males in the sample who may be described as residing in category 1A. The rest of the figures are read in the same way.

The model makes three kinds of errors: (1) There are some people in Kilaloni who actually reside in ways not covered by the elicited residence categories (i.e., children living with their mother's brother or with their father's mother, if he and his wife are both away). Although all these situations make sense to my informants and can be explained in



Adjusted for double-counting and wrong descriptions, 94.5% of the males are correctly allocated.

FIGURE 3. Tree diagram of ordered questions leading to men's residence categories.

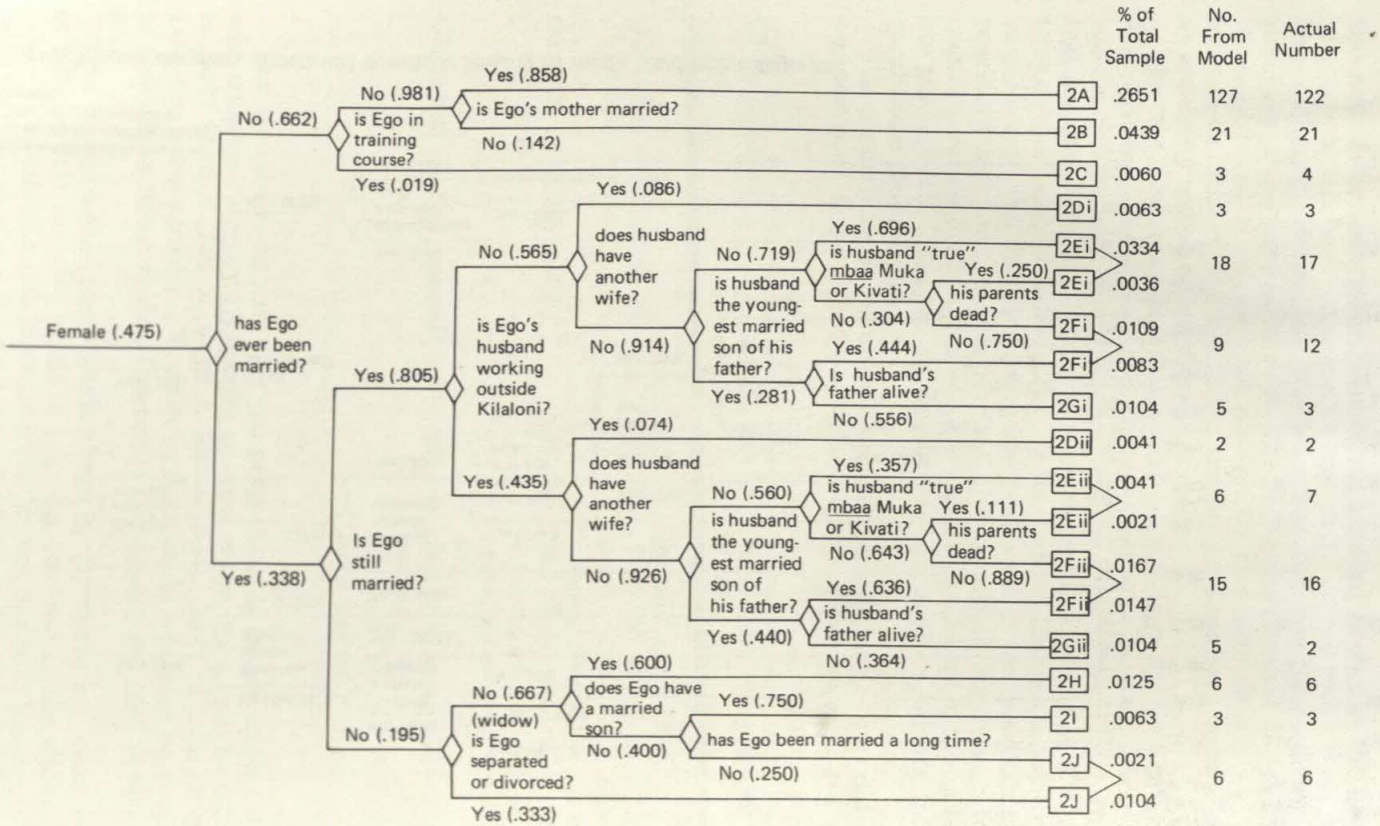
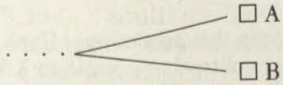


FIGURE 4. Tree diagram of ordered questions leading to women's residence categories.

an *ad hoc* fashion, they do not seem to be "usual" enough to have been originally elicited. These people invariably share all of the social status characteristics associated with one of the residence categories in the model. The problem is that they have additional ones and so are put in that category by mistake. (2) There are some mistakes that the model counts twice. Suppose that we have twenty men and two residence categories and, further these men are actually divided evenly between the two categories. Now suppose that the end state of the model looks like this:



	Predicted	Actual
□ A	11	10
□ B	9	10

The numbers show an absolute value discrepancy of two, one in each case. What has happened, however, is that only one man from category B has been misassigned to category A. The model makes only one mistake rather than two. (3) The third type of error misplaces four men. It is like the second error, but it does not show up in the final figures. Two men who are actually in category Y are put by the model into category Z, while two men from Z are put into Y. As far as the final assignments are concerned, these moves cancel each other. These errors have to be added to the error total.

When a count of mistakes is made and adjusted for double-counting and cancellation, it turns out that the model correctly assigns 94.8 percent of the people to their actual residence categories. In other words, one can pick any of the 480 individuals in the sample, feed him into the system, and nineteen out of twenty times he will end up in the right residence category. Moreover, the fact that different paths may lead to the same category takes into account the possibility that people may be residing in a particular way for more than one reason.

RUMINATIONS

No claims are being made that the model has been shown to be successfully predictive. In order to make such a claim, one would have to take the ordered sets of questions to another Akamba community and see if they correctly assign a new group of people to the proper residence categories. This is a purely descriptive model. It suggests that concatenations of personal status characteristics are associated with indigenous residence categories in a patterned way.

Describing the residence patterns of a group of people in terms of their own categories offers some insights into social organization that are not necessarily available when traditional ethnological categories are used. Moreover, the accuracy with which the model can utilize the information Kilaloni Akamba consider necessary to describe how particular

people ought to be living is a mark both of the cohesion of the system and of their ability to understand it, and talk about it.

NOTES

1. An earlier draft of this paper was presented at University College, Nairobi, December 1969, when the author was a participant in the University of East Africa Social Sciences Council Conference. My fieldwork at that time was being supported by the Child Development Research Unit, Harvard University and University College, Nairobi, as well as by NIMH Grant MH 12039–01 CUAN.

   I would like to thank George Collier, Roy D'Andrade, Charles Frake, Naomi Quinn, Janet Shepherd, Thomas Weisner, and John Whiting for aid and commentary.
2. Three informants were interviewed using this procedure. Each one was asked to discover the residential circumstances of twelve people in Kilaloni. A fourth individual was used as a check on the elicitation. He was presented with bundles of personal status characteristics derived from the interviews and asked to describe how a person with these characteristics might reside.
3. Essentially, information theory is brought in here more as a metaphor than as a rigorous mathematical structure. The mathematical machinery of information theory is somewhat more complicated than this presentation would have one believe. For instance, the question of subjective probabilities is begged here. In order to use information, an individual brings with him or her some personal sense of what the real world looks like. In the kin term tree, the universe consists of eight labels, each of which, as a label, is equiprobable in the domain. If we present someone with the task of choosing one of the labels through questioning, we can probably assume that that person's subjective probabilities about the distribution of the labels is the same as the objective probabilities.

   If we are interested in the actual people in an informant's life referred to by these labels, however, we cannot know how much a given piece of information is worth unless we know the informant's own notions about the distribution of real relatives in the real world.

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20

A Natural System Used in Mfantse Litigation Settlement

NAOMI QUINN*†

Anthropologists have become increasingly interested in decisions as units for the analysis of social structure, social change, and economic systems. Most of the natural decision-making situations described by these anthropologists are of the sort which cognitive psychologists have variously called choice among "complex" (Tversky 1972:297), "composite" (Tversky 1972:294), "multi-attribute" (Shepard 1964:264) or "multi-dimensional alternatives" (Tversky 1969:40), or "compound consequences" (Lee 1971:96). Characteristically, such choice is between several alternatives, which differ with regard to several criteria, each having different and incommensurable value. I can best exemplify this kind of choice to anthropologists with some common situations faced by inhabitants of the coastal Mfantse fishing town of Biriwa, near Cape Coast, Ghana, where the research reported in this paper was carried out.[1]

As a first example, a Biriwa man who marries a woman of another town faces the difficult choice of whether to live in his wife's town or his own. Married couples ordinarily reside in separate houses, and a woman is unlikely to leave her matrilineal kinswomen, with whom she lives, to be near her husband in another town. He must balance the advantages of living close to his children, who will grow up among their matrilineal kin, against the advantages of living among his own matrilineal kin and long-time friends and acquaintances, who represent, among other things, opportunities for membership in their various fishing crews. It is because Mfantse men recognize this particular dilemma that most of them try to avoid it by marrying endogamously. A more strictly economic example, involving multiple choice criteria, is that of a Mfantse fishing boat owner deciding whether to go on a migratory fishing expedition. He may have to weigh the possibility of a heavy catch, the opportunity to amass capital during the six or eight weeks away from kinship obligations in his home town, and his capital requirements in terms of present indebtedness and plans for new equipment against his crew's unwillingness to travel and his own political and ceremonial commitments at home during the coming season. Still another example, involving more than two alternatives as well as several criteria in terms of which these alternatives must be assessed, is the decision of a Biriwa fishmonger to sell her smoked fish at her smoking ovens, or at the Cape Coast market, or at an inland market. She must make this choice on the basis of several considerations, chiefly travelling distance, risk of spoilage, and expected profit, itself a complex criterion based on both estimated fish supply and estimated demand. This particular choice for Mfantse fishmongers has

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† Source: *American Ethnologist* 3: 331–351, 1976 (revised).

been investigated by Hugh and Christina Gladwin (Gladwin and Gladwin 1971; C. Gladwin 1975) in the city of Cape Coast. Examples like these, involving choice among complex alternatives, abound in ethnographic description, because they abound in real life, both the lives of the people we study and our own.

Anthropologists who investigate areas of life in which multiattribute decisions arise would, understandably, like to be able to go beyond a description of the decisions to be made, the alternatives open to members of a society at each decision point, and the criteria which these actors use in selecting among these alternatives. They would like to be able to describe the decision-making process itself.[2] Attempts to make models of this process are motivated by the belief that the way in which people make decisions affects decision outcomes just as distinctly as the various demographic, economic, normative, and other constraints on these decisions.

This paper represents an effort to characterize the way in which people make naturally occurring decisions. Fundamental to this effort is the assumption that people manage to make complex decisions by first reducing them to simpler form.

SIMPLIFYING HEURISTICS FROM PSYCHOLOGY

Even experimentally induced decisions can be complex, and some cognitive psychologists have called attention to the heuristics used by experimental subjects to simplify such decisions. Slovic and Lichtenstein (1971:713), for instance, have characterized the performance of decision makers under certain experimental procedures as "a cognitive shortcut employed to reduce the strain of mentally weighting and averaging several dimensions at once." Similarly, Tversky (1972:297) has concluded from experimental evidence:

> When faced with an important decision, people appear to search for an analysis of the situation and a compelling principle of choice which will resolve the decision problem by offering a clear-cut choice without relying on estimation of relative weights or on numerical computations.

While psychological research into the simplifying heuristics individuals employ has been scattered and incomplete to date, several different kinds of heuristics have been identified and described (Quinn n.d.). I will detail only two such contributions from psychology, which relate most closely to the model of Mfantse litigation settlement I am going to present.

The first is Simon's relatively early and well-known formulation of the "satisficing" principle (1957:204–205), by which individuals search among available alternatives until they find one which is satisfactory. This alternative is then chosen, although there may well exist other, more desirable alternatives in the universe which have not yet been searched out. By settling for the first satisfactory alternative he encounters, an individual saves himself the costs of searching further. These costs may be physical, such as the time and effort a Mfantse fisherman might expend searching for a perfect log in the forest, from which to carve a new

canoe. Or they might be purely "mental," such as the costs of recalling and sorting through a large amount of information about a large number of alternatives. To cut down on the mental costs of information processing, a decision maker may limit the number of criteria he uses to judge alternatives, as well as the number of alternatives he considers. This point is important, because in the model of Mfantse litigation settlement to be described, the number of alternatives is relatively small, but the number of criteria which could influence selection among these alternatives is potentially endless.

A second relevant suggestion psychologists have made about the way people manage complex decisions is that they frequently limit their calculations to consideration of only one criterion at a time. In the literature of cognitive psychology, criterion ordering is a feature of the "lexicographic model" (Tversky 1969). In this model, the decision maker applies criteria one at a time in order of their judged importance. Those alternatives are chosen which are best with regard to the first-ordered criterion; if two or more alternatives are equivalent with regard to this criterion (or, in a version of the model called "lexicographic semi-ordering," if they are so similar that they are judged equivalent [Tversky 1969:32]), the second-ordered criterion is then introduced for selecting among them, and so on until a single alternative remains. In a modification of the lexicographic model which Tversky (1972:285) calls "elimination by aspects," he has proposed that no prior fixed ordering of criteria be assumed. Rather, at each stage at which a new criterion must be introduced to differentiate the remaining alternatives, the decision maker is thought to select this new criterion with the probability proportional to its weight, or importance, relative to the other criteria remaining. This formulation allows for the notion that, at different moments in time, the decision maker's state of mind, with respect to the relative importance of criteria, may be different. Lexicographic ordering and elimination by aspects both posit ordering devices which relieve a decision maker of the numerical computation which would be required to estimate the exact weights of all the relevant criteria at once.

The model of litigation settlement I am going to present will incorporate both of the general principles described above: that is, litigation settlement is simplified by limiting the amount of information which bears upon a particular settlement and by applying this information in terms of one criterion at a time. This model will differ from psychological models like those for satisficing, lexicographic semi-ordering, and elimination by aspects in that it describes, not a heuristic adopted by individuals to make decisions on their own, but a culturally instituted system for making one kind of frequently recurring decision.

MFANTSE LITIGATION SETTLEMENT

I will first sketch the way in which Mfantse litigation proceeds, although my interest in this research is focused on only one small part of these proceedings. Most cases of litigation which arise in Biriwa are settled before a "panel" of four or five elders; except in rare cases such as murder

or when a chief is destooled, Biriwa matters do not reach government courts. There are eight or nine standing panels in the town. Particular panels may decline in reputation and popularity, and new ones may take their place, organized by men who, having learned litigation settlement as members of someone else's panel, begin to settle cases on their own, calling together for the purpose several other men whose judgment they respect. Sometimes, but not always, a man chooses close friends or members of his own matrilineage to work with him. He calls this group of men together whenever a case is brought before him; not everyone needs to attend the hearing of every case, and substitutes are frequently called in to take the places of those who are away from town or otherwise unavoidably absent. The resulting panel, which comes to have a stability of membership and a reputation of its own for settling cases, is known by the name of its organizer. Certain kinds of cases are handled by special panels—for example, the fishing chief's panel, composed of representatives from each clan in town, hears all litigation involving fishing incidents; but, for the most part, people bring complaints to the panel of their choice, and certainly some panels have better reputations for judicious settlement, and fuller agendas, than others.

Cases are most frequently heard on Tuesdays, which is a tabooed day for fishing, and Sundays. The complainant, through a go-between, obtains the defendant's agreement to bring their case before a given panel, and a time and place is set. When all panel members, parties to the grievance, and potential witnesses are assembled, the complainant and defendant, respectively, are allowed to present their sides of the case. Then each has the opportunity to address questions he thinks are pertinent to the other, any witnesses are called and questioned by both sides, and the panel asks whatever additional questions it feels are necessary. The panel's questions are delivered by the "linguist" *(kyeame)*, a panel member chosen from among themselves as spokesman; other members are free to break in with questions if they wish. When the panel is satisfied that it has heard all the facts of the case, and the case cannot be summarily dismissed, it retires to another, private room to deliberate.

This private discussion takes place in two stages. First, the panel determines which of the two parties is at fault. Determination of fault seems to be most often based on an actual admission by the offender, or lacking such an admission, guilty behavior such as evasion of questions or surliness during the presentation of the case. In some cases, of course, the guilty party dissembles successfully, or both parties sincerely believe in their positions, and fault must be determined on the basis of a legal interpretation of the case.

Next, the panel must set a fee which the offender is ordered to pay in order to pacify the offended party. This stage of the discussion may take as long as the determination of fault, or longer. Having determined who is at fault, and what pacification fee this party must pay, the panel decides, finally, on a small charge for their services, based on the length of time the settlement required, which is payable by both disputants to each panel member.

The panel then returns to the hearing to announce its decision and

to give any advice with regard to reconciliation and behavior reform deemed suitable. At this stage the panel's decision is unalterable, and the reasons for this decision are not revealed. However, the offender is free to beg a reduction of the pacification fee from the offended party, who, in turn, may offer to accept a lower pacification fee or refuse the pacification fee altogether, in the interests of reconciliation. It is also customary for disputants to beg a reduction of the settlement charge and for the panel to reduce or waive this amount. Each disputant also pays a small fee to any witnesses he has called, and the offender gives the panel a bottle of *apeteshie* (homemade gin costing –/8/–), with which a libation is poured.

A case so settled can subsequently be appealed to another panel. However, decisions are rarely appealed; appeals are most likely to be made when a losing disputant suspects bias against him, or, more commonly on these all-male panels, her. In other cases, disputants abide by the panel's decision, although one party may then express his dissatisfaction with this decision, or his unwillingness to accept any ultimate resolution of the dispute, by divorcing or otherwise severing relations with the other party to the case.

SETTING PACIFICATION FEES

Pacification fees have traditional names and are also referred to by amount in terms of the old British sterling system used in colonial times. These amounts, which are always discussed in sterling, are paid in Ghanaian currency, at a conversion rate of two *cedis* to a pound; thus, a shilling equals ten Ghanaian *pesewas*, and a sixpence is treated as five *pesewas*. The amounts of pacification fees range in value from –3/9 (37½ *pesewas* or about 33 U.S. cents) up to 8/8/–(C16.80, or about $15); but high pacification fees are infrequent and are almost invariably reduced, and most cases result in payments of 2/–/6 (C4.05, or about $3.50) or less. The nominal amounts of these pacification fees, and the common practice by which recipients return the pacification fee to its donor in a gesture of reconciliation, suggest that the payments have more symbolic than economic significance. Nevertheless, the question of the pacification fee is very seriously debated by the panel. Three, or sometimes more, different pacification fees, sometimes representing a total difference of less than one pound, may be considered, and reasons supporting each presented, before a settlement is concluded.

In order to investigate how panels actually set pacification fees, I interviewed three elders, each the organizer of a panel, and all regarded as highly expert in matters of litigation. In the course of a number of interviews, I learned from these men about a common system which they used, with minor variations between panels, to set pacification fees. I also gathered from them a number of actual cases, which had been settled by their respective panels within the past two years or so, and I discussed with them the way in which the system for setting pacification fees applied to each case. One of the elders who contributed to this research, Nana Kwesi Kakraba (Panel A), was able to provide me with twenty-nine such

cases, in which traditional pacification fees had been set, which had been heard by himself and his panel; Kyeame Kobina Ampah (Panel B) narrated an additional thirteen such cases, and Apofohene Kofi Tawiah (Panel C), eleven cases.[3]

All the panels work with a ranking of the traditional pacification fees. There is no disagreement among panels about the amounts of these fees, nor, since rank depends upon relative amount, about their rank order. The values of the pacification fees fall at unequal intervals, although one subset of six pacification fees, perhaps a fragment of an older system, is pegged at intervals of 2/–/6 with half-steps of 1/–/3. Three other pairs of pacification fees are also based on ratios: one member of each pair has twice the value of the other (Table 1). Whatever their origins, all pacification fees are treated by the panels as members of a single rank-ordering which ignores the intervals between ranks. This fixed rank-ordering is a central feature of the system for setting pacification fees (see below).

TABLE 1. The Values of Pacification Fees.

preguan	8/8/–
esuasa	6/1/6
esuono na sur (*esuono* and a half)	5/1/3
esuono	4/1/–
osua na sur (*osua* and a half)	3/–/9
osua	2/–/6
sur (half)	1/–/3
esian	1/7/–
nsan	–/13/6
guinea	1/1/–
guinea ni fa (half-*guinea*)	–/10/6
ageratwi	–/9/–
dadakɔr	–/4/6
branbran (a bottle of brandy)	–/5/6
apeteshie (a bottle of gin)	

The pacification fee schedules used by the three panels vary slightly (Figure 1). Panel A is the only one which uses the pacification fees called *esian* and *esuono na sur*.[4] The rank-ordering of pacification fees employed by Panel C extends both above and below those of the others; it includes a very high fine, *preguan*, which other elders whom I interviewed claimed was a special kind of customary payment not ordinarily used in litigation; and at the other end of the scale, Panel C recognized the possibility of letting an offender off with *apeteshie*, the customary bottle of illicit gin which he must give to the panel. This practice, which is tantamount to reducing the pacification fee to nothing, is also known as "giving a gift"; other elders argued that it is better to require some pacification fee, however small, than none at all. Panels B and C treat some pacification fees which are closely equivalent in value as alternatives to be used inter-

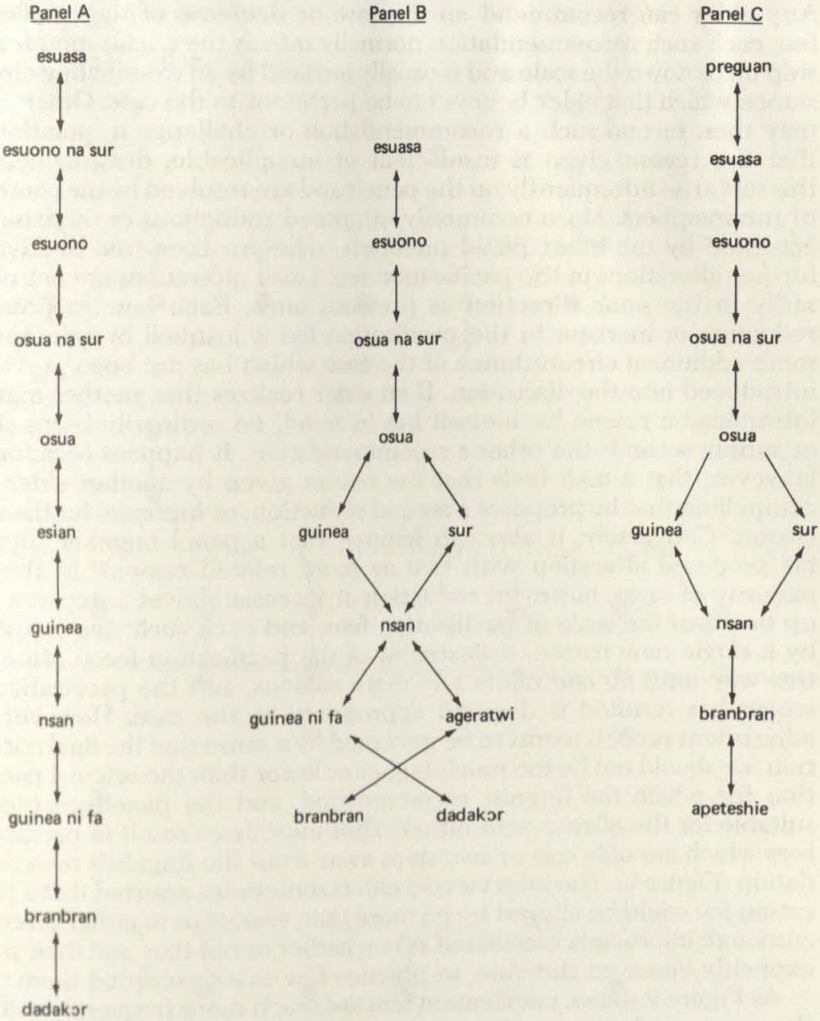

FIGURE 1. The ranked pacification fees used by three panels.

changeably, depending on which happens to be mentioned first in a particular case.[5] It is likely that alternative usages are introduced into a panel by members who have previously served on other panels using different pacification fee schedules. Ordinarily, however, elders are unaware of what goes on in panels other than their own, and under these circumstances, there is an interesting degree of variation.

Despite these variations in the fees used, the process by which a pacification fee is determined in each case is much the same on all three panels. This process is initiated by the linguist. He makes an initial recommendation, which serves as a point from which discussion can take off and further reductions or increases can be suggested by panel members.

Any elder can recommend an increase or decrease of the pacification fee; each such recommendation normally moves the pacification fee one step up or down the scale and is usually justified by an extenuating circumstance which that elder believes to be pertinent to the case. Other elders may then second such a recommendation or challenge it, pointing out that the reason given is insufficient or inapplicable; disagreements of this sort arise infrequently on the panels and are resolved by the consensus of the members. More commonly, proposed reductions or increases are accepted by the other panel members, who are then free to advocate further alterations in the pacification fee. Later alterations are not necessarily in the same direction as previous ones. Each new proposal for reduction or increase in the pacification fee is justified by reference to some additional circumstance of the case which has not been previously introduced into the discussion. If an elder realizes that another man has introduced a reason he himself has in mind, he ordinarily keeps silent, or simply seconds the other's recommendation. It happens occasionally, however, that a man feels that the reason given by another elder is so compelling that he proposes a second reduction, or increase, for the same reason. Conversely, it also can happen that a panel member supports his proposed alteration with two or more related reasons. In the vast majority of cases, however, reduction or increase moves a step at a time up or down the scale of pacification fees, and each such step is justified by a single new reason. Adjustment of the pacification fee continues in this way until no one offers any more reasons, and the pacification fee which has resulted is deemed appropriate to the case. However, the adjustment process seems to be governed by a sense that the final pacification fee should not be too much higher or lower than the original pacification fee which the linguist recommended, and the panel accepted, as suitable for the offense committed. Thus most cases result in pacification fees which are only one or two steps away from the linguist's recommendation (Figure 2). The interviewed elders sometimes asserted that a pacification fee could be altered by no more than two steps in either direction. Although informants vascillated as to whether or not they and their panels explicitly observed this rule, in practice few cases exceeded them.[6]

As Figure 2 shows, pacification fees are much more frequently reduced than increased. Commonly given reasons for reduction are that a case is between a husband and wife, close relatives, or close friends. The litigating elders view reconciliation as an important part of their job, and they know that high pacification fees create hard feelings. On the other hand, in some cases increases in the pacification fee are argued on the grounds that the offended party, who is highly incensed over the offense, will not be satisfied with less. Other reasons for reducing or increasing pacification fees reflect the panel's concern to appear unbiased. In one case of wife-beating (C3), it emerged that the beating had been much more severe than that which another man had given his wife in a similar case (C2) heard by the same panel a year earlier. Because of the severity of the beating in the later case and the history of quarrelling in that marriage, the linguist set the initial pacification fee at *esuono* rather than *osua* as in the first case. However, in the subsequent discussion, the pacification

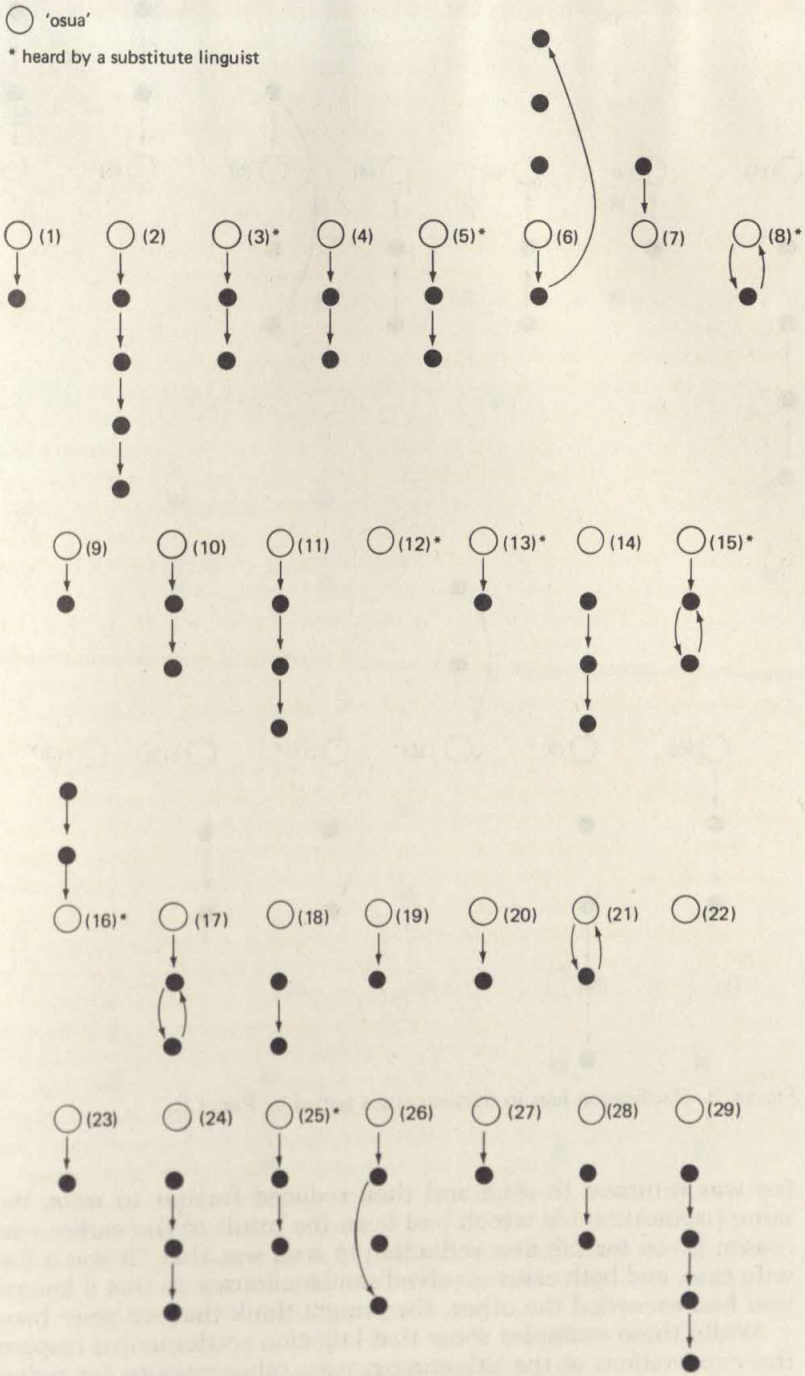

FIGURE 2. Pacification fees in twenty-nine cases settled by Panel A.

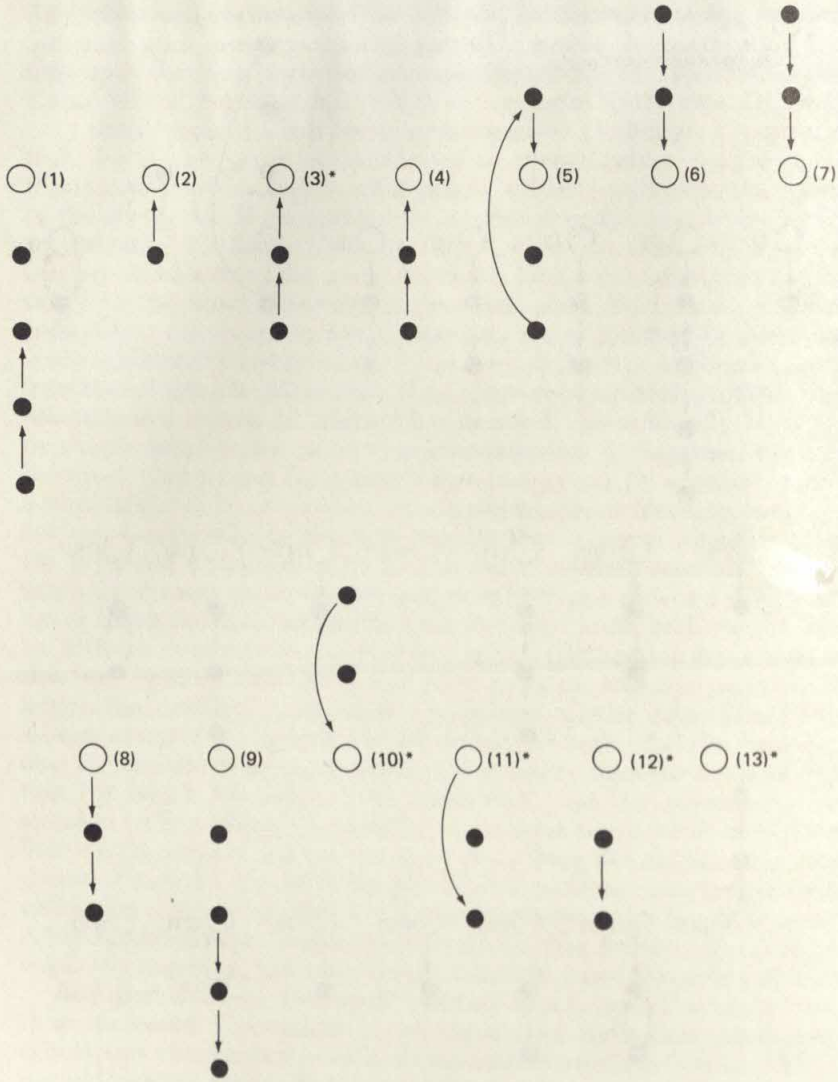

FIGURE 3. Pacification fees in thirteen cases settled by Panel B.

fee was returned to *osua* and then reduced further to *nsan*, the very same pacification fee which had been the result of the earlier case. The reason given for the first reduction to *osua* was that "it was a husband-wife case, and both cases involved similar offenses, so that if one pacification had exceeded the other, they might think that we were biased."

While these examples show that litigation settlement is responsive to the expectations of the litigating parties, other reasons for reducing or

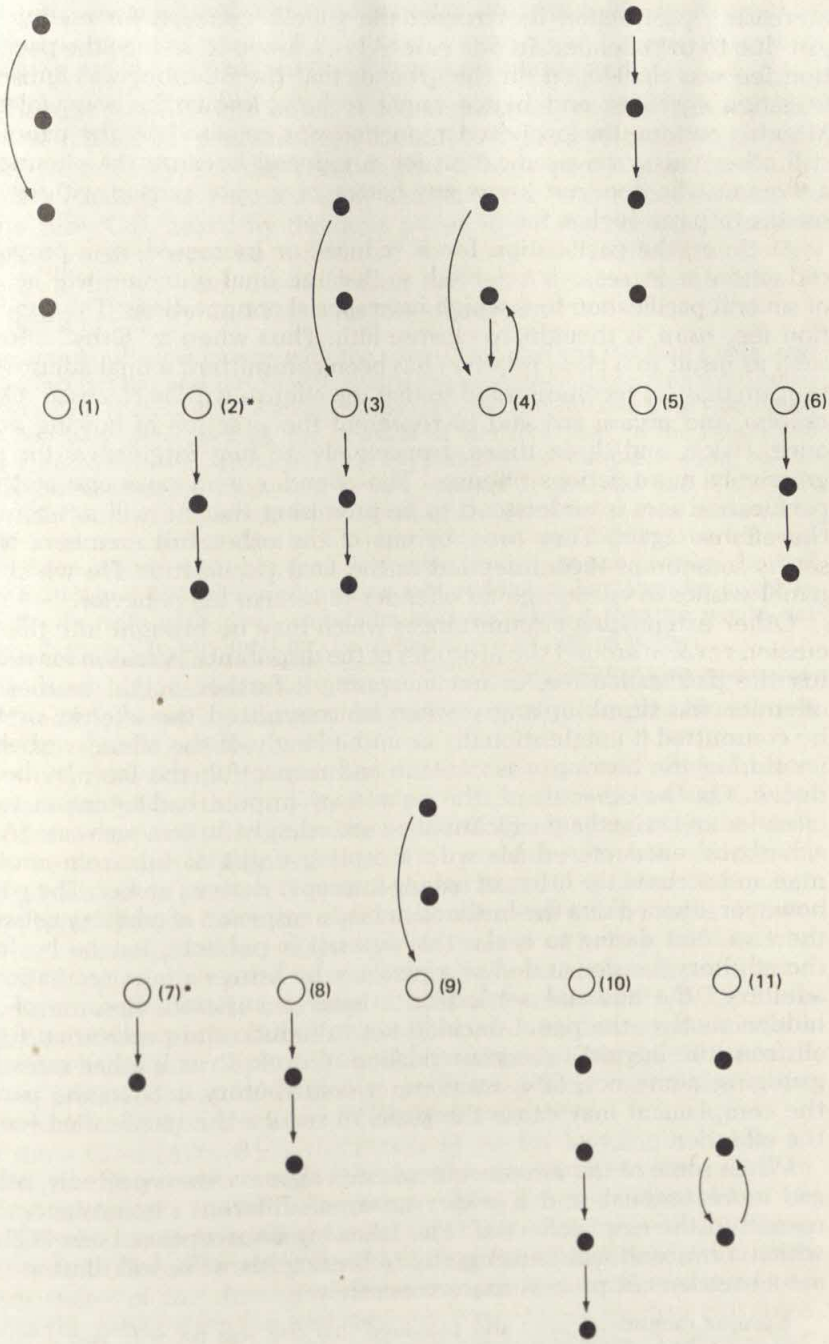

FIGURE 4. Pacification fees in eleven cases settled by Panel C.

increasing pacification fees reflect the panel's concern for meting out just due to the offender. In one case (A17), a move to reduce the pacification fee was challenged on the grounds that the offender was himself a litigation specialist and hence ought to have known he was violating Mfantse custom; the proposed reduction was rejected by the panel. In still other cases, the pacification fee is reduced because the offender is a woman who does not know any better, or a poor person without the means to pay a higher fee.

At times, the pacification fee is reduced or increased, or a proposed reduction or increase is rejected, so that the final outcome will be one of several pacification fees which have special connotations. The pacification fee, *nsan,* is thought to cleanse filth. Thus when a "filthy" offense, such as insult to a close relative, has been committed, a final adjustment to *nsan* may be recommended so that the offense will be cleansed. *Osua, esuono,* and *esuasa* are said to represent the practice of bowing down once, twice, and three times, respectively, to beg forgiveness for progressively more serious offenses. The offender who pays one of these pacification fees is understood to be promising that he will not commit the offense again. Thus *osua,* or one of the other full members of its set, is sometimes recommended as the final pacification fee when the panel wishes to encourage an offender to reform his behavior.

Other extenuating circumstances which may be brought into the discussion revolve around the attitudes of the disputants. A reason for reducing the pacification fee, or not increasing it further, might be that the offender was drunk or angry when he committed the offense, or that he committed it unintentionally or unthinkingly. If the offender's behavior during the hearing was contrite and respectful, the fee may be reduced. On the other hand, the panel may impute bad intention to an offender and raise the pacification fee accordingly. In one such case (A12), a husband encountered his wife accepting a gift of fish from another man and accused the other of having improper designs on her. The panel, however, divined that the husband nursed a suspicion of adultery between the two. Not daring to make this accusation publicly, lest he be fined the adultery fee demanded of a person who brings a false accusation of adultery,[7] the husband settled on a lesser accusation. Because of this hidden motive, the panel decided not to reduce the pacification fee at all from the linguist's recommendation of *osua.* In still other cases, begrudging demeanor, false motives, or contributory acts on the part of the complainant may cause the panel to reduce the pacification fee for the offender.

While some of the circumstances cited above arise repeatedly, others are more unusual; and a wide variety of different circumstances are named in the cases collected. The following unexceptional case (C2), in which a husband was found guilty of beating his wife, will illustrate the actual settlement process more concretely:

> *Kyeame* mentioned *osua* and reasoned that she was his wife and if on one occasion he had beaten her, he ought to promise to refrain from that behavior. Then Kwa Fynn reduced it to *sur* with the reason that he did not want the marriage to end and wanted to better the relationship. Kofi Tewiah reduced

> it further to *nsan* with the reason that in Kwesi Yabo's marriage there had been no dispute before, and if it had been a marriage with a series of troubles then we would have realized that they had no interest in it.

The linguist justified his initial recommendation, in this case, on the grounds that an offense had been committed, and two reductions were made, respectively, on the grounds that a high pacification fee would hurt the chances of reconciliation and that this was a first offense. A second case (C4), heard by the same panel, illustrates the way in which opposing reasons may be introduced to move the pacification fee up and down. In this case, the defendant had been accused by his wife of committing adultery and had admitted doing so.

> *Kyeame* mentioned *esuono* with the reason that she was the man's wife, and if he had left her and flirted with another woman, then he never gave respect to the wife. Then Kobina Asoku said that this was true, but that if they accepted it then they were seeking the downfall of the marriage; in the future the man would throw it up to her that she had wanted the pacification, so the pacification must be *osua*. Kofi Tewiah opposed it and said that if she was not satisfied that she would be annoyed, so the pacification must be *osua na sur*. From *osua na sur* then Kwa Fynn rejected it and said that the husband had been staying with her for a long time, and if on one occasion he had been attracted by a different woman, and yielded to her, then he ought to pacify the wife with *osua*, and if the wife accepted it then he would get enough money to buy food for the children. We all agreed to that.

In the course of this discussion, the pacification fee was raised, lowered, raised, and lowered again, providing an unusually complex illustration of the adjustment process.

Figure 2 shows that, in a minority of cases, the reduction of pacification fees does not follow this step-by-step pattern; rather, as in Case C4 described above, pacification fees are reduced by skipping over the next lower fee directly to the one below. This practice, which is recognized by all panels but is practiced excessively by Panel C, is called "jumping" or "splitting." Jumps down the equal-ratio subset of which *osua* is a member, from *esuasa* to *esuono* (skipping over *esuono na sur*) or from *esuono* to *osua* (skipping over *osua na sur*) are known as "removing one *osua*." Jumps from *esuono* (4/1/–) to *osua* (2/–/6) or from *esian* (1/7/–) to *nsan* (–/13/6), or from one to the other member of any other equal-ratio pair result in a halving, or "splitting" of the pacification fee. Splitting seems to be a preferred way of reducing pacification fees: five of the seven jumps which occur in the cases I collected are splits.

In some cases (A26, B10), the reason given for jumping is simply to shorten the discussion. Jumping is also the device most often used to readjust a linguist's initial recommendation when this is thought to be extremely inappropriate. Most jumps, six of the seven recorded in these cases, are introduced to adjust the linguist's recommendation, rather than at later stages of the discussion. Since the panel normally increases or reduces the pacification fee, one step at a time, not more than two steps up or down the scale from the initial recommendation it adopts, the linguist who successfully convinces his panel to start very high or very low can, theoretically, alter the final settlement more than other panel

members. However, the members are quick to bring their linguist's recommendations into line when they judge them inappropriate. If a linguist recommends an inordinately high or low pacification fee, he may be suspected of bias in the case, and the others may raise or lower it to a more moderate level. The regular linguist on Panel B, who presided over most of the cases I collected from that panel, was a new apprentice to the role. Unlike the other linguists, he seemed not to have yet evolved any consistent rule for setting the preliminary pacification fee: his recommendations were highly variable. They were frequently corrected by other panelists, who were likely to suggest that the pacification fee he had named was in error and might be adjusted by skipping directly to a higher or lower one. One example is Case B5, in which a man admitted to having made improper advances to another man's wife:

> *Kyeame* mentioned *nsan;* he made a mistake. Then Kobina Ampah said that this case never deserved *nsan*, for the reason that it seemed to be a murdering case. Then Okwentsir mentioned *osua na sur* with the reason that he wanted to be sure that the husband (who was the offended party) would never nurse a grudge against the proposed suitor and provide a gun duel or a knife duel.

Exceptionally, in this case, the pacification fee was adjusted upward by skipping three steps to the fourth. The new pacification fee was then treated as the point of departure for a subsequent reduction of one step.

Experienced linguists base their preliminary recommendation on the nature of the offense committed, as the linguists' pacifications in Cases B2 and B4 above illustrate. A linguist who is unsure about the class of offense involved in a case, or about the pacification appropriate to that class of offense, may fall back on precedent. He will search his memory for an earlier case he has heard which is as nearly comparable as possible to the case under consideration, and he will set the preliminary pacification in the current case equal to the final pacification in the remembered case.[8] Such a resort to precedent is likely to raise panel discussion about differences between the two cases, and indeed panel members themselves frequently introduce other, similar cases for purposes of comparison. However, unlike the linguist's initial recommendation, which is supposed to be based on the offense itself, the panel's subsequent arguments for reductions or increases in this preliminary pacification fee are normally supported by citing extenuating circumstances in the case.

While the description and model in this paper focus on the latter process, something can be said to clarify the way in which linguists arrive at the initial recommendation from which subsequent adjustment departs. *Osua*, a moderate pacification fee, is sometimes characterized as being "behind all cases" of litigation and hence the appropriate preliminary pacification fee for most kinds of cases.[7] The linguists on Panel A clearly observe this rule: they almost always begin by recommending *osua*. Other linguists more frequently treat certain offenses, such as severe wife-beating, failure to perform customary rites, serious insults and accusations, offenses involving an entire clan, and adultery on the part of a man (unlike a wife's, not subject to a special adultery fee) as more serious than others, and deserving of a higher pacification fee. This pacification

fee is most often *esuono;* the regular linguist on Panel C seems to reserve a still higher pacification fee, *preguan,* for the most serious offenses of all. In the first of two such cases (C1),[7] the offending wife had behaved so as to invite suspicion of adultery, and in the second case (C5), a man had accused a clanswoman of being a slave.

Because of the modest number of cases sampled and the unsystematic sampling of those cases which involved more "serious" offenses, only ten such cases are represented among those I collected, and the list of offenses resulting in such cases is suggestive rather than exhaustive. The classes of offense treated as serious, and the levels of pacification fees appropriate to these more serious offenses, were not explored with the current linguists themselves. It seems likely that linguists on different panels, and even different linguists substituting on the same panel, use different rules to set the initial pacification fees for serious offenses. The elders whom I interviewed were just as leery of generalizations about the way in which the preliminary pacification fee was set, as they proved to be unwilling and inconsistent when encouraged to generalize about many other aspects of the pacification fee system. Just as other features of the system were drawn out of the discussion of specific cases, and only then verified by these informants, so the manner in which each linguist sets preliminary pacification fees would have to be elaborated in the course of discussion, with the linguist himself, about his treatment of a number of actual cases.

While the linguist's preliminary recommendation is based on the nature of the offense committed, he may also make use of his prerogative, as a member of the panel, to introduce the first reduction or increase himself. In such cases he may state the nature of the offense, the pacification fee it deserves, and then go on to mention a mitigating circumstance which justifies alteration, usually a reduction, of the pacification fee. Case C8 illustrates this exercise of the linguist's dual role:

> When *Kyeame* mentioned *osua* there was *esuono* behind it, and he left it and mentioned *osua* with the reason that when a wife called a husband a fool the pacification was *esuono,* but they were looking after children, and only the husband would be able to pay the pacification although he was a winner, so the pacification should be moderate to meet the pocket of the husband in order to insure a good marriage.

More often in such cases, the linguist does not explicitly name the pacification fee appropriate to the offense, but opens discussion directly with a suggestion for reducing the pacification fee. Further discussion then departs from the linguist's recommendation. While additional reductions are likely to result (Figure 2), the panel may also reverse the effect of the linguist's recommendation by increasing it. This is in fact the response of Panel B to some of its linguist's recommendations, which were deemed inappropriately low (B1, B2, B3, and B4).

Thus the three panels differed somewhat, during the period of time in which they litigated these cases, in the level at which different linguists tended to open discussion of the pacification fee and the tendency of the panel to adjust the linguist's initial pacification fee by jumping or

reversing it. Some of these differences can be accounted for by the extreme variability of Panel B's apprentice linguist. In addition, in the small sample of cases collected, Panel C's linguist seemed more inclined to treat offenses as serious and assigned them higher initial fees. Since Panel C regularly "split" such recommendations, the final outcomes of these cases were not inordinately high, and average outcomes on the three panels converged closely. The differences which did obtain between these panels, which hear much the same types of cases, can be thought of as differences in panel styles. However, these stylistic differences should not obscure the common principles on which this system for setting pacification fees operated in all three panels.

SIMPLIFYING PROPERTIES OF THE SYSTEM

In formal summary, the system can be described as one in which a fixed rank-ordering of a set of possible alternatives—the pacification fees—is adopted, one of these rank-ordered alternatives is selected as a point of departure, and one-step increments or decrements (and more rarely, two-step jumps) from this point are optional. I think that this system can best be understood in terms of human cognitive requirements. In three respects, it reduces the cognitive burden of making otherwise highly complex decisions.

First, the system permits litigation specialists to satisfice—to reduce each case to a small number of the most salient facts about it. What makes litigation settlement complex is not the array of offenses which are brought to be litigated; Mfantse elders treat most of these as equivalent in degree, singling out some as a class of more serious offenses. What is unmanageable is the enormous number of differing circumstances surrounding cases. Using this system elders need not consider, exhaustively, all the potentially relevant circumstances which combine to characterize each individual case and make it unique; they need only consider those three or four or five facts of the case which stand out as most salient. Contributing further to the limitation of possibilities is the fixed rank-ordering of pacification fees; after a preliminary pacification fee has been established, each subsequent step of the adjustment process normally requires that only two pacification fees adjacent in rank be compared at a time; at each step, the panel must agree either to move the pacification fee one more step, or keep it where it is.

One elder observed that a man who wishes to settle cases has to look for three or four friends to help him. Eventually I realized that the system for setting pacification fees depends upon the independent perceptions of several observers to produce several different "salient" reasons for reduction or increase. A single individual might be susceptible to a particular bias, whereas the viewpoints of several different panel members insure that a wider and more balanced set of salient reasons will be introduced into the settlement. Thus, while I originally worried that the cognitive process I wanted to study would be complicated by processes of social influence, I eventually realized that the system was not designed

to accommodate group processes; rather, the group was assembled to make the system work.

Secondly, the system employs an ordering device which relieves litigation specialists of the necessity of directly comparing different salient reasons which enter into the discussion of a case. Elders do not have to weigh their reasons, one against another, to estimate how much relative weight each should contribute to the settlement under discussion. Rather, reasons are considered one by one, and each reason is accepted or rejected independently. The real money values traditionally assigned to pacification fees are ignored, and only their ordinal positions are employed, so that each reason for a move from one pacification fee to the next contributes equal weight to the final outcome. This weight is positive if the reason given favors increase and negative if it favors reduction. The pacification fee which is finally agreed upon depends upon the initial departure point adopted and the sum of positive and negative reasons which move the pacification fee upward or downward from this point. This procedure has the same effect as the individual decision-making heuristics described above—lexicographic semi-ordering and elimination by aspects—techniques which similarly eliminate the need to compute the exact relative weights of the relevant decision criteria by processing one criterion at a time.

Finally, because a traditional system exists for the making of this kind of recurrent, complex decision, each new case can be settled with a minimum of time and effort devoted to the invention and acceptance of decision procedures. Members can agree from the outset on the way to settle cases; and indeed, some panel members can participate in the settlement of litigation without having fully learned, or without ever fully understanding, the system for setting pacification fees. This cognitive division of labor, in which one or two apt and experienced elders in each panel deliver expert opinions about difficult cases and correct erroneous recommendations by other panel members, seems to be the rule in these proceedings. Thus natural systems for performing complex cognitive operations which recur frequently in a society may be viewed as adaptations to human cognitive limitations, and the differential distribution, across individual society members, of such cognitive skills as humans are capable. Whenever complex decisions or other complex cognitive tasks recur in a society, natural systems for simplifying these tasks can be expected to evolve. Such systems are adaptive in a sense usually ignored by anthropologists; they are adaptive not to ecological constraints or those of the human personality, but to constraints on human cognition.

METHODOLOGY

Although the system I have described simplifies decision making, it should not be assumed that the system itself was easily discovered. The elders interviewed were, by reputation, the most expert litigators on their respective panels and perhaps the most expert in town; nevertheless, they were unable to give any straightforward account of how the system

worked, as I have explained it here. At first, attending to their frequent mentions of "murdering" cases, "disgrace" cases, "false allegation" cases, and other classes of cases, I assumed that these broad categories would hold the key to the way pacification fees were determined. But the categories in which the cases fell proved inconsistent with the pacification fees set. If I inquired why *osua* was charged in one case of false allegation, but not in another, similar case of the same sort, I was inevitably told, "Each case has its own pacification." Next, turning to the elders' frequent comments that such-and-such a case was an *esuono* case or an *nsan* case, I tried to elicit lists of cases for which each pacification fee was appropriate. In response to such questions, elders began to supply contingencies: for instance, I learned that a case in which a husband insulted his wife in public would be an *osua* case, but if he insulted her in the privacy of their room it would only be a *sur* case; a case in which a husband neglected to give his wife food money was *nsan,* but if he failed to give her money for six months or more, it was *osua,* and so on. I tried to collect lists of all relevant contingencies, but they were endless, and each time another contingency was introduced to explain why two seemingly identical cases were treated differently, Kwesi Kakraba would explain patiently, "Each case has its own pacification." When two elders were asked about the appropriate pacification fee for a certain kind of case under a certain set of contingencies, they almost always disagreed; and if I confronted them with the disagreement, each would disavow any knowledge of "how they settle cases over there" on the other's panel.

Finally one day I followed up a remark which gave me a clue to the whole system; one of the elders mentioned that a certain pacification fee had been "reduced." I shortly discovered the procedure, and then the basis, for reducing and increasing pacification fees. At first I asked elders to tell me, in hypothetical terms, what could be reduced or increased to what. They made frequent errors in the rank of pacification fees which led me to try to construct somewhat complex and idiosyncratic models of each elder's system. Eventually I realized, and Kofi Tewiah confirmed this, that they were simply trying to perform the unaccustomed memory exercise of listing all the pacification fees one or two steps above and below a given pacification fee, as questions such as "What can *osua* be reduced to?" and "What can *osua* be increased to?" demanded. Kwesi Kakraba commented one day that "This is a very hard thing to do, because we don't usually do it this way."

Throughout the remainder of the research, the elders varied in their ability to articulate and sometimes their willingness to endorse the principle that *osua* was behind all litigation and the definitions of "jumping" and "splitting" and other features of the system I have described. I had to verify each of these principles in the actual cases reported to me. However, the elders *were* able to narrate these cases in great detail, including details of who said what during the setting of the pacification fee.

Undoubtedly, it is because natural systems for decision making and carrying out other cognitive tasks are not readily verbalized, that such systems have been overlooked by field workers. Anthropologists using

traditional field techniques, including those of ethnoscience, have always relied heavily upon native labeling, normative statements, and other kinds of readily verbalizable data. In contrast to practitioners of ethnoscience, I did not start out by asking, "What can people talk about?" and "How do they talk about it?" Rather, my concern was, "How do people execute a relatively complex cognitive task?" Methods for investigating natural cognitive systems are not well developed; mine could probably be best described as a "hook-or-crook" technique. After I managed, by hook or by crook, to figure out how the system worked, I was then able to verify my model of it against the descriptions of actual cases. Sometimes, apparent conflicts with the data led me to modify my model; for instance, I discovered that some pacification fees I had thought were ranked were actually equated, and I was able to verify this usage with my informants. Other apparent discrepancies in my model turned out to be real exceptions to the system, as, for instance, the special connotation accorded to the pacification fee, *nsan*, or the extreme variability of one apprentice linguist's recommendations.

At the same time, I learned that while I could not rely upon my informants to tell me how the system worked, I also could not afford to ignore the verbal clues they gave me, especially if they dropped these clues over and over again (Miller, Galanter, and Pribram 1960:126; D'Andrade 1970:132). In particular, I came to understand that the repeated explanation, "Each case has its own pacification," was an accurate claim and a key feature of the system. Since the system did not require panels to consider all the multitude of contingencies potentially relevant to any case, but only those reasons which were salient to panel members, the setting of each pacification fee was a unique event. Not only might each case be settled differently from any other case, but it might be settled differently, by that particular panel, on that particular day, than it would have been by another panel or the same men in other states of mind. This fact was borne out by my informants, who were always able to supply, in our post mortems of cases, new reasons for the pacification fees which had been set, to add to the ones brought up during the actual settlement.

At its most general, my method can be characterized as one of concept formation—trying out a series of tentative principles until I finally found the one that worked, in the sense that new cases fit it and informants verified it. This is probably the very same method employed by novices like Panel B's apprentice linguist in learning how to litigate. Like this man, apprentices may operate for relatively long periods of time with erroneous or partial understandings of the litigation system. They can correct their models of the system, as I did, only by attending to new cases which come along, searching for regularities in the settlement of these cases, trying out new rules to account for these regularities, and verifying these rules. Because of the way I was accustomed to learn, I continually tried to short-cut this concept-formation task in my own research; I found it hard to believe that my informants could not teach me the rules of litigation settlement directly and explicitly. Once, I asked Kobina Ampah how he taught apprentices to litigate, and whether he

could not teach me the same way. He answered that apprentices learn by hearing cases.

It is my impression that many tasks in non-Western, non-school settings are learned the way in which Mfantse men learn litigation settlement.[8] In the acquisition of such traditional skills, the learner is expected to extract relevant principles from a number of examples of these principles at work; he is not given the principles, nor is the teacher accustomed to state them in general form. A methodological suggestion emerges from this research into one traditional system taught in this manner. Anthropologists investigating complex cognitive systems might better work with, rather than against, indigenous teaching methods, by systematically and self-consciously utilizing concept-formation heuristics to discover how such systems work.

NATURAL SYSTEMS OF DECISION MAKING

The Mfantse system for litigation settlement simplifies decision making in several ways. From the description and model of this system which has been presented in this paper, it is possible to begin characterizing natural decision-making systems of this sort in more general terms.

The process whereby salient features of a case supply reasons for stepwise adjustments of the pacification fee closely resembles a principle, identified by Geoghegan (1973:268–298) in portions of the Philippine Samal address term system he studied, which he termed a "marking rule." The operation of this rule is illustrated by one segment of the address term system, in which speakers may opt to communicate "cues" on three expressive dimensions. The "departure-point" in this system is the speaker's habitual name for the addressee; the speaker can express positive or negative affect, deference or its lack, anger, or a combination of these messages, by moving up or down from habitual usage, on natural scales composed of the addressee's name, nickname, pet name, and name substitutes.

Geoghegan speculates that marking rules of the sort used in Samal address are adaptive in the sense of being cognitively efficient. The cues which are input into such rules occur infrequently, a person's habitual name being almost always used. Under this condition, to search for appropriate cues through the entire repertoire of possible cues on each new occasion for usage would be extremely wasteful of cognitive effort. Instead, users of the rule can utilize information on these three expressive dimensions only when such feelings become salient and omit expressive messages altogether, when these feelings are neutral, by adopting their habitual usage. They need not search through their various feeling states each time they use an address term, because the system allows them an "unmarked" term, one which does not make any expressive statement.

The Mfantse system for setting pacification fees in litigation proceedings is similar to the Samal marking rule described for address terms; it also eliminates the need for exhaustive search, in this case, through the contingencies which may relate to a given case. In the Mfantse system I have described, however, the efficiency or simplification achieved by

delimiting search is due to the fact that the number of potential reasons for increasing or reducing a pacification fee is so large; not, as in the Samal address term example, because the cues for altering one's habitual usage pop up so infrequently. Natural decision-making systems which employ saliency in place of systematic search may be of wider generality than either of these two systems alone suggest.

Also of potentially wide generality in natural decision-making systems is the feature of this system by which decision criteria are ordered and dealt with one by one. Tversky (1972) has argued that elimination by aspects, which also processes criteria one at a time, accounts for some kinds of individual decision-making data better than additive models. This strategy seems to be designed to eliminate direct comparison of disparate criteria and the computation of their relative weights on a common numerical scale which such comparison would require. While it accomplishes the same simplification as does elimination by aspects, the Mfantse system achieves this simplification quite differently. Tversky's model eliminates alternatives by successive applications of the next criterion, until sufficient criteria have been introduced to eliminate all but one alternative. The order in which criteria are introduced can make a difference, since the alternative chosen on the bases of earlier criteria might have been eliminated on the basis of some later criterion. The Mfantse system moves up and down a fixed ranking of alternatives until all the salient criteria are exhausted. All these criteria contribute to the final outcome, which is the sum of positive and negative criteria; by a convention of the system, all contribute equally. What is common to both elimination by aspects and Mfantse litigation settlement is that both order the relevant criteria. Both appear to be special instances of a more general cognitive strategy for simplifying decisions.

Finally, the principle that "Each case has its own pacification," and the latitude in actual outcomes which results from reliance on saliency to delimit the facts of a case, suggest something else about the system for setting pacification fees. It is a system designed less to insure a certain maximum or optimum decision, as simply to facilitate the making of a decision. As Tversky (1972:298) points out, avoidance of weighting and computation in favor of principles which yield clear-cut choices "may lead to very poor decisions." But within a broad range of defining what is a socially appropriate or acceptable pacification fee the elders who settle cases in Biriwa are protected from the consequences of their decisions. Litigants may be affected by these outcomes, whenever, for instance, an unsatisfactory pacification fee precipitates lasting bad-feelings or the break-up of a marriage. Even so, such consequences are hardly so disastrous, or so directly attributable to a single decision, as are many economic outcomes. And it can be argued, in a functional vein, that the expedient settlement of disputes, by accepted authorities using legitimate litigation techniques, is more critical to the life of the community than the precise degree of fairness or satisfaction attained by each settlement. In contrast, anthropologists investigating recurrent economic decisions and other decisions on which survival literally hangs may find that the systems evolved for making such decisions are capable of defining

and approaching much more optimal solutions. To arrive at "best" solutions, such systems may employ principles very different from those used in Mfantse litigation settlement and other systems in which simplification, not optimization, is the goal. In any event, the investigation of natural systems for decision making of all kinds, and all kinds of natural cognitive systems, has hardly begun.

The most general implication of this paper for the study of cognition is that decision making, and performance of other cognitive tasks, can best be understood in terms of a two-stage process. The first stage involves selection of an appropriate structure, or representation, upon which to perform the task, and the second stage involves operations on that structure. As D'Andrade (1979:4) has written,

> . . . the majority of cultural language-based algorithms do not specify precisely how a task is to be accomplished. . . . Of course there are exceptions; for example, in our culture, doing long division consists of carrying out a precise set of routines. However, the more typical situation is one in which the dominant part of the algorithm is the formulation of a representation of the problem or situation. . . .
>
> Much of cultural information processing seems to operate by giving the person a very general goal and a number of potential frameworks by which the situations can be represented. Once a representation is accomplished, specific routines can then be employed to attempt to accomplish the goal.

In the case reported here, an enormous simplification of the decision-making task is achieved by the acceptance of a unique task representation, the traditional system. While it has come to diverge in detail from small group to small group, this representation is standardized across groups in its main features. It is reasonable to assume that many other recurrent cognitive tasks which individuals accomplish are performed on such culturally shared knowledge structures.

NOTES

1. Support for the field research on which this paper is based was provided by NIMH Small Grant Number MH 25814-01, a Duke University Faculty Summer Fellowship, and a stipend from the Duke University African Studies Committee. I am deeply grateful to Nana Kwesi Kakraba, Kobina Ampah, and Kofi Tawiah for their participation in this research, and to Erskine Justice Amoah for his intelligent and unfailing assistance in carrying it out. I am also grateful to Carol Smith and Harry Levy for useful comments on an earlier draft of this paper.
2. The motivation to devise models of decision making, for instance, is clear in the recent history of the study of social structure, which I have reviewed elsewhere (Quinn 1975).
3. Although the latter man was the fishing chief of Biriwa, and thus responsible for the litigation of fishing cases, he and his panel also heard an array of other litigation. I instructed him to recount only these other cases, in order to make the sample of cases collected from him comparable, in the range of offenses litigated, to the cases narrated by the other two elders.
4. The other two panels recognize neither of these pacification fees, although they employ other members of the equal-interval subsets: *esian* is paired with

nsan, half its value, and *esuono na sur* is the half-step between *esuono* and *esuasa.*

5. Thus Panel C regards *guinea* (1/1/–) and *sur* (1/–/3) as equivalent in rank; Panel B regards this pair and two others equivalently: *guinea ni fa* (half-guinea, or –/10/6) and *ageratwi* (–/9/–); and *branbran* (brandy, at –/5/6 a bottle) and *dadakɔr* (–/4/6). Of course, it is suggestive that *guinea* and its half-value are terms borrowed from an earlier British monetary system; possibly they were government court fines, introduced, as Kwesi Kakraba speculated, into the traditional system of pacification fees some time during the colonial period. Panel A uses these British terms, not their alternatives, and rank-orders *branbran* and *dadakɔr* instead of making them equivalent; Panel C uses neither *guinea ni fa* nor *dadakɔr.*
6. A case heard by Panel C (C3), described below in the text, was reduced by a total of four steps to make its outcome comparable to that of an earlier, similar case heard by the same panel. Several cases heard by Panel A resulted in reductions of more than two steps due to a special connotation of the pacification fee, *nsan. Nsan* is a symbolic substitute for the expensive custom of slaughtering a sheep, and, like sheep sacrifice, it is thought to have a cleansing effect on offenses which are somehow "filthy." In Case A29, no such offense had been committed, and Kakraba and his panel wanted to avoid such an implication lest they anger the offender and thus lessen the chance of reconciliation. In Case A2, between a husband and wife, the offending wife was extremely annoyed and had threatened divorce in the event that she was charged a high pacification fee. Consequently, the panel reduced it by an unusual third step to *nsan* and then had to reduce it still another step to avoid *nsan.* In Cases A11 and A26, on the other hand, the panel reduced the pacification fee more than two steps because they felt that filthy offenses had been committed—in the former, a man had accused his classificatory "father" of theft, and in the latter, an entire clan had been offended. The two other panels recognized this connotation of *nsan* but did not consistently adjust their settlements to accommodate it.
7. Some kinds of cases require special pacification fees which are not included in the rank-ordering of other pacification fees and cannot be reduced. Two of the three elders considered *preguan* (8/8/–) to be such a pacification fee, appropriate only to "murdering cases" in which, by actual assault, negligence, curse, or witchcraft, the complainant's life is endangered. Another special pacification fee is *ayerfar,* the adultery fee of 5/–/–, which is usually accompanied by a smaller additional pacification fee called *dunsa* (–/5/–) and two bottles of *apeteshie.* This is the fee which a man must pay to the husband of a woman with whom he has committed adultery. If, on the other hand, either of these types of offenses is falsely alleged, the accuser must compensate the falsely accused person with precisely the pacification fee appropriate to the offense itself. In order to ask for the adultery fee in such cases of false allegation, the linguist must be able to argue that an allegation of actual adultery with a specified person has been made. General accusations against spouses for adulterous or loose behavior are common and are treated like other insults and false accusations. In one case, for instance, (C1), a husband accused his wife of visiting another man in his room, secretly, without permission, at a suspicious hour. Because the two were surprised in innocent conversation rather than sexual intercourse, the man was not required to pay the adultery fee. The wife, although she maintained that she had gone to the man's room for the sole purpose of discussing a private matter, was charged the high pacification fee of *esuono* (reduced from the linguist's recommendation of *preguan*) for her indiscretion. In another interesting case (A6) a man failed to substantiate

his claim that his wife had been unfaithful. He was found guilty of accusing her falsely, and the linguist recommended *osua*. This was reduced to *esian*, but at this point the panel decided that the offense was really tantamount to a false accusation of adultery; the husband had intended such an accusation, although he was unable to identify the man he claimed to have seen leaving his wife's room. The pacification fee was changed to *esuono na sur*, because this fee (5/1/3) was equivalent to the adultery fee. The wife accepted this settlement, but insisted that she also be paid *dunsa* and two bottles of *apeteshie*.

8. Linguists' options for setting the preliminary pacification fee were not fully described in the original version of this paper because they were not fully understood at the time it was written. A return field trip to Biriwa in 1975–1976, funded by National Science Foundation Grant No. SOC75–19204, provided me with an occasion to clarify this part of the fee-setting process in discussions with Kobina Ampah.
9. This impression comes from my own field observations and those related to me by several other field workers. A striking published example is Nash's (1958) description of newly hired workers learning their jobs in a Guatemalan factory. Of course, there are certainly exceptions to this method of learning complex tasks, like the training program of Puluwatan master navigators described by T. Gladwin (1970), which involved verbal relay and rote memorization of a large number of "recipes" for island-to-island navigation and related sea lore.

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21

Arithmetic as a Tool for Investigating Relations Between Culture and Cognition

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Arithmetic problem-solving activity is found in all cultures, and arithmetic is so richly analyzed a domain that it is extraordinarily well suited to helping solve some of the problems encountered in educational and cognitive research.[1] Establishing the comparability of a given activity among cultures, among subcultures, among educational institutions, between everyday tasks and experimental tasks, between individuals, or among the problem-solving activities of a single individual requires detailed description of the activities in which people engage. Mathematical notation for arithmetic provides a tool for describing even minute differences in problem-solving behavior. More important, the systematic properties of the arithmetic domain make possible powerful inferences about the problem-solving processes that are taking place when particular problems elicit particular responses. Few domains of human activity (language being a notable exception) are formal enough to offer this opportunity.

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† Source: *American Ethnologist* 6:568–582, 1979.

The problems to which this paper is addressed grow out of interrelations between cognitive psychology and anthropology, but in crucial ways it is the contribution of the mathematician that makes possible the program of research we describe.[2]

THE PROJECT

Our research strategy has been to utilize what might be called "naturally occurring experiments" to control many of the extraneous factors that cannot be anticipated and directly controlled through experimental procedures. Thus, we have compared the impact of two educational settings on a single group of people within a single culture. Broad features of the culture were the same for all and were therefore held constant; the group of individuals was held constant, while we examined relationships between their varied educational experiences and their varied problem-solving skills. Additional constraints were placed on the range of variation by the choice of the two forms of education, which were alike in some ways and contrasted in others.

We compared traditional tribal apprenticeship with formal Western schooling among Vai and Gola tailors in Monrovia, Liberia. A crucial part of the enterprise was specifying the similarities and differences between the two forms of education, drawing on anthropological theory as well as our own observations (Middleton 1970; Fortes 1938; Firth 1936; Mead 1943; among others. Gay and Cole 1967 and Ginsburg 1977 are studies of arithmetic in Liberia and in the United States). We then looked for a domain of knowledge that was learned in both settings. Arithmetic was one of the major subjects taught in Liberian schools. And tailors, with or without schooling, used arithmetic to measure customers and lengths of cloth, to construct patterns on the cloth, and to sew clothes, as well as to carry out financial transactions with customers, suppliers of cloth and buttons, and rent and tax collectors. Therefore, arithmetic seemed a promising domain for comparing the impact of apprenticeship and of schooling on cognitive skills.

Traditional apprenticeship was chosen because it has both fundamental similarities to Western schooling and certain differences from it, which have been viewed by psychologists as important in producing different cognitive performances by "school" and "nonschool" experimental subjects (for example, Hall 1972; Scribner and Cole 1973; Wagner 1974; Brown 1977; see Serpell 1976; Cole, Sharp and Lave 1976 for extended discussions of the issue).

Both schooling and apprenticeship last five or more years, long enough to have significant impact on the skills, habits, and strategies of the learners. Both lead to the transmission of complex and extensive bodies of knowledge, and this volume and complexity surely affect the strategies learners develop for learning. Both involve the explicit understanding that the learner is there primarily to learn, which may affect how much conscious monitoring he/she does of the educational process and what sorts of metaskills develop out of the educational experience. The two forms of education involve individuals of comparable age ranges and

hence can be presumed to confront comparable ranges of developmental variation in learners.

The two forms of education differ in the social organization of teaching and learning. For one thing, apprenticeship takes place in the arena of mature practice of the skills the apprentice is trying to learn—an apprentice learns tailoring in the tailor shop—whereas schooling occurs in a setting quite divorced from it. The amount of formal teaching may also vary, with somewhat less in the tailor shop. This may have an impact on learner's models of how to communicate with others about their skills and may also affect the level of abstraction at which principles to be learned are introduced to the learner. Schooling and apprenticeship vary in the degree to which instruction is shaped to the particular needs of the individual learner, apprenticeship providing much more of this.

Our research was carried out in Monrovia's Tailors' Alley, where as many as 250 tailors and apprentices may be found within a single block. Although not all the tailors are of the closely related Vai and Gola tribes, the members of the sample we worked with most closely were of these tribes. The twenty tailor shops—small, wood, dirt-floored, zinc-roofed buildings—cater to the sartorial needs of the poorest segment of the city's population. Tailors both sew for customers and make for sale ready-to-wear trousers. Their inventory ranges from one or two pairs of trousers to thirty or forty pairs, most tailors maintaining a stock of ten to twenty. They have all come to Monrovia from villages in the interior, and it is their jobs that have brought them to the city. A master tailor usually has one or two apprentices at a time and may over a period of ten years produce three or four masters. None of the tailors has come to Monrovia with the primary goal of attending school; indeed, it is in no way seen as a necessary part of learning the tailoring profession. Many, however, have attended school at one time or another, and probably more do so in Monrovia than would have in their villages because of the greater accessibility of schooling in the capital (see Cole, Sharp, and Lave 1976 for discussion). Apprentices who are attending school spend their mornings there and the rest of the day—until 7 or 8 o'clock at night—in the shops. Among the tailors, the length of tailoring experience ranges from a month or two up to thirty or more years; the length of school experience ranges from none to tenth grade. The fact that we found tailors with all combinations of these two kinds of educational experience allowed us to focus our research efforts on this one community, whose members shared long acquaintanceship, similar life histories, a single work environment, and their daily lives.

The research involved participant-observation and informal interviewing in the shops of Tailors' Alley in order to learn about the apprenticeship process, the body of knowledge that the apprentice must master, the business of tailoring, and the general features of daily life. After some months of this sort of effort, we rented a small section of one tailor shop and used this space for interviewing 140 of the tailors and apprentices in considerable depth and then carrying out a series of experimental tasks with subsamples of the tailors. The interviews covered apprenticeship history and school history, family background, a modernity of atti-

tudes scale, selection procedures that might affect the channeling of individuals into the tailoring profession, and beliefs about teaching and learning. The experimental tasks were aimed at establishing the distribution of tailoring skills among those with different levels of experience and at establishing both arithmetic skills and verbal access to principles of tailoring. Analysis of these tasks has been comparative and has attempted to establish the relative amount of learning transfer as a result of apprenticeship and school experience. We will discuss one of these tasks in detail.

THE ARITHMETIC TASK

In the course of administering an early arithmetic task, we found that the tailors were quite articulate about the steps they took in solving arithmetic problems. This led us to explore the possibilities of arithmetic as a language for describing problem-solving practices. It looked like a good way of moving beyond simple performance comparisons to deeper questions about the cultural particulars of problem-solving activities.

The decision to go more deeply into problem-solving processes led to a new set of trade-offs. On the one hand, the more systematic the sampling of the domain of formal arithmetic, the easier it is to apply theory about relationships between problems and responses to particular instances of problem-solving behavior. On the other hand, arithmetic problems that are representative of the domain of arithmetic are a very unrepresentative sample of the arithmetic problems tailors encounter in their daily lives.[3] We did not want to give up either the ability to specify relations between test problems and problems encountered in daily life or the ability to draw inferences about problem-solving processes from systematically designed and theoretically crucial contrasts between problems. Our solution was to sample the two domains in separate experimental tasks. The task we will describe here sampled the formal domain. It was an arithmetic test that included several sets of problems varied systematically in an attempt to sample a broad range of skills.

The test was given orally, in the tailor's first language, to one tailor at a time. The tailor could respond in his first language or in English, as he chose, and was free to use a variety of calculational aids, consisting of a measuring tape, a large pile of buttons, and pencil and paper. We took notes on the procedures used in solving the problems, including the use of these aids. Also, we asked the tailors to explain their steps as they solved different problems and noted their explanations. In analyzing the data, we looked very carefully at the types of errors made on a particular problem and compared these with errors on certain other problems chosen because of their relations to the focal problem. We also looked at an individual's errors on one problem and predicted the pattern of that individual's errors on other problems. We then tried to confirm this pattern.

Thus, problems were designed to sample all four arithmetic operations—addition, subtraction, multiplication, and division—at five levels

of numerical difficulty: 1) one-digit numbers; 2) two-digit round numbers; 3) other two-digit numbers; 4) three-digit round numbers; 5) other three-digit numbers. Numbers and an operation were combined to produce the "skeletons" of the problems at each level. Other criteria were used to assign additional context to the problem, for example, creating word problems with roughly the same degree of familiarity to the tailors in their daily arithmetic activities. Table 1 shows a matrix of problems arrayed by numerical difficulty and by operation. A sample problem is presented for each level. Another series of problems utilized this matrix but was phrased in monetary terms. Two problems were added, so that 23 + 8 was presented both as \$23 + \$8, and as \$2.30 + \$.80, while 139 − 20 was presented both as \$139 − \$20 and as \$1.39 − \$.20. A further series of problems held numerical difficulty constant and explored the impact on performance of increasingly unfamiliar problem material, and a final series varied along an abstractness scale. These last two series are discussed and analyzed elsewhere (Lave n.d.).

TABLE 1. Structure of the Arithmetic Task.

Numerical Difficulty Level	Sample Problem	Arithmetic Operation			
		+	−	×	÷
1	Each pair of trousers needs three buttons. How many do you need for six pairs?	3 + 4	10 − 6	3 × 6	12 ÷ 3
2	You put five pockets in a Higher Heights suit. How many pockets would there be in five suits made the same way?		40 − 20	5 × 5	60 ÷ 10
3	Six pairs of trousers each need four buttons. How many buttons do you need?	23 + 8	65 − 9	6 × 4	18 ÷ 3
4	Forty tailors each made five pairs of trousers. How many trouser were there altogether?		400 − 200	40 × 5	
5	It takes three buttons for one pair of trousers. How many buttons are needed for twenty-eight pairs?		139 − 20	3 × 28	

THE CONCEPT OF AN ARITHMETIC SYSTEM

To speculate about the particular problem-solving processes used in arriving at the solution to a test problem requires, we found, basic information about what we have come to call the "arithmetic system" being used by the problem solver. An arithmetic system is composed of a set of number names (a numeration system) and a set of strategies for performing addition, subtraction, multiplication, and division. Lest this sound simplistic, we would add that most people use several numeration systems on a daily basis; that most people use several strategies for performing arithmetic operations; and that the numeration system may or may not affect the strategies.

Some sort of numeration system is available to every known culture. We ourselves have at least three: spoken English, written English, and written Arabic. Any given number name in one of these three systems has a corresponding name in the others and stands for the same discrete quantity. Thus, it is irrelevant whether we enunciate "fourteen" or write 14; our audience will know the precise quantity we have in mind. This multiplicity of ways of representing numbers occurs in many cultures; it certainly occurs in any culture in which members are multilingual or know how to write. One of the major features of multiple numeration systems is that, though they represent the same concepts, they involve both distinct symbols and distinct rules of grammar for relating those symbols to produce number names. As a consequence, a given individual may use numeration systems that are similar and perhaps reinforcing or dissimilar and mutually interfering.

There are two classes of strategies for performing arithmetic operations, those that deal with quantities as such and those that deal with number names. Strategies that work with quantities are universal and manifest themselves as counting on fingers, manipulating pebbles and the like, or using an abacus. The tailors make extensive use of counters—either movable ones, such as pebbles, or marks on paper. Strategies using number names are typified by the Western algorithmic manipulations learned in school, for example, the "put down the 4 and carry the 1" litany in addition. In such treatment it is the manipulation of symbols, in a very real sense divorced from reality, that carries the burden of computation. Besides the algorithms we are acquainted with in our culture, there are many others throughout the world, each tied to a particular numeration system but transferable to related systems.

Contrary to the impression all too often given by mathematicians and teachers of mathematics, arithmetics have not sprung up Athena-like, fully developed and ready to attack daily problems. In fact, they have slowly evolved as people have found better and more efficient ways to solve certain kinds of recurrent problems. We shall here consider four arithmetic systems—the Vai/Gola, the Western school-taught, and the monetary version of each of these—in terms of their similarities and differences and the way in which these affect the tailor's performances on our experimental task.

VAI/GOLA AND SCHOOL-TAUGHT DISCRETE-QUANTITY ARITHMETIC SYSTEMS

Vai/Gola arithmetic makes use of the numeration system presented in Table 2. It is a spoken, not a written, numeration system.[4] Because Vai and Gola seldom use numbers larger than 20, 20 is a sort of upper bound for everyday numeration. This is corroborated by the fact that 100, 5×20 in Gola, is in Vai expressed, not in terms of the generative system, but by *hundo lundo*, clearly a borrowing of English "hundred." There are distinct symbols for the first five integers, for 10, and for 20. Other numbers are formed from these by simple rules of combination: the systems are additive (17 is $10 + 5 + 2$, Vai *tanso(olu)fela)* and multiplicative (60 is 3×20, Vai *mosakpakbande*). Conceptually the system is vigesimal, in the sense that any number can be expressed in terms of powers of 20. However, the extensive use of subtallies at 5 and 10 and the fact that Vai and Gola rarely if ever make use of the higher power (20^2,

TABLE 2. Vai and Gola Numbers and Their Glosses.

Vai	Gola	Gloss
lundo	*gũ*	1
fela	*tie*	2
sakpa	*tai*	3
naani	*tiina*	4
soolu	*naana*	5
soolundo	*die gũ*	5 + 1
soofela	*die tie*	5 + 2
soosakpa	*die tai*	5 + 3
sonaaní	*die tiina*	5 + 4
tan	*zia*	10
tanlundo	*zia ye beh gũ*	10 + 1
tanfela	*zia ye beh tie*	10 + 2
tansakpa	*zia ye beh tai*	10 + 3
tannaani	*zia ye beh tiina*	10 + 4
tansoolu	*zia ye beh noonu*	10 + 5
tansolundo	*zia ye beh noonu die gũ*	10 + 5 + 1
tansofela	*zia ye beh noon die tie*	10 + 5 + 2
tansosakpa	*zia ye beh noonu die tai*	10 + 5 + 3
tansonaani	*zia ye beh noonu die tiina*	10 + 5 + 4
mokbande	*gbao*	20
mokbande a ko lundo	*gbao ye beh gũ*	20 + 1
mofelakbande	*gbao tie*	2×20, that is, 40
mosakpakbande	*gbao tai*	3×20, that is, 60
mosakpakbande a ko tan	*gbao tai ye beh zia*	$3 \times 20 + 10$, that is, 70
monaanikbande	*gbao tiina*	4×20, that is, 80
hundo lundo	*gbao noonu*	100 once (Vai); 5×20 (Gola)

$20^3, \ldots$) makes it somewhat misleading to call the system base 20. Indeed, the word for 20^2 is specifically not vigesimal, but decimal *(hundo naani)*. Similarly, the intrusion of 5 and 20, with no occurrence of $10^2, 10^3, \ldots$, rules out calling it a decimal system. For like reasons, neither is it base 5. Zaslavsky (1973) calls such systems quinquavigesimal, ignoring the occurrence of 10 as a tally.[5]

In many important ways, the school-taught numeration system is like the Vai/Gola system. It, too, uses a small number of symbols that are combined additively ($17 = 10 + 7$) and multiplicatively ($60 = 6 \times 10$). As in the Vai/Gola system, larger quantities are written before smaller ones (that is, hundreds precede tens, which in turn precede units, just as in Vai/Gola twenties occur first, then tens, fives, and units). It differs from the Vai/Gola system in being strictly base 10 (in particular, in having no special notations for 5 and 20) and in using zero as a place holder.

Vai/Gola arithmetic operations either are "done in the head" or involve the manipulation of sets of markers. In view of the fact that the Vai/Gola number names force the speaker to view numbers larger than 5 as composites, not as entities, we were not surprised to find the tailors frequently using grouping and tallying strategies. In particular, we found many cases of groupings by 5, 10, or 20. As with any system, recurrent combinations, especially small sums or multiplications, are automatically memorized, and, indeed, all but the most inexperienced young apprentices gave prompt, memorized solutions to the simpler problems on the test.

The school-taught strategies involve the writing of numerals in aligned columns and the use of decimal place-holding algorithms in which tallying occurs in the guise of borrowing and carrying from one column to another. Operational symbols ($+, -, \times, \div$) are introduced; frequently the plus sign is omitted from addition problems. Algorithms in general are learned by rote, and this tends to obscure the underlying principles involved.

Because users of the Vai/Gola system seldom make use of large numbers, we expected them to have difficulty in working problems involving large quantities. We expected that wrong answers might be off by small amounts, because of the prevalence of counting and tallying strategies. Furthermore, many problems gave them opportunity for losing track of their tallying if they were performing the operations in their heads. For example, when we add 17 and 3, we have a single "carry the 1" to do: Vai and Gola have two analogous steps to perform: $(10 + 5 + 2) + (3) = 10 + 5 + 5 = 10 + 10 = 20$. Not only do these extra "carrys" make problems more convoluted, but they are likely to lead to answers off by 5, 10, or 20—on the high side for subtraction, on the low side for addition. Another difficulty we anticipated was in multiplying 40. Because 40 is expressed as 2×20, multiplying it involves multiplying a multiple of 20, and we imagined that Vai and Gola might lose track of the number of multiples of 20 actually obtained as the multiplications proceeded. For somewhat similar reasons, we expected problem solvers to lose track of sub-subtractions in problems involving complicated Vai/Gola numbers such as in the $65 - 9$ problem, which translates as $(3 \times 20 + 5) - (5 + 4)$.

We expected test takers who were using Western, school-taught arithmetic to make errors in borrowing and carrying, producing answers too high by 10 in subtraction or too low by 10 in addition. Further, we expected reversal of digits and misuse of zero as a place holder because of confusion of its roles as place holder and numeral.[6] Also, of course, we expected wild answers from people who had not yet mastered the algorithms or who had misrecalled arithmetic facts such as the multiplication tables.

VAI/GOLA AND SCHOOL-TAUGHT MONETARY ARITHMETIC SYSTEMS

American bills and coins constitute the official money of Liberia. The tailors use pennies, nickels, quarters, half-dollars, and bills. Though extensive coinage is initially introduced to reduce the number of coins required for any given transaction (for example, substituting a quarter for five nickels or two dimes and a nickel for twenty-five pennies), the effect is to emphasize certain preferred fractional parts of the basic dollar unit. These fractions—half, quarter, tenth, and twentieth—become perceived as a new entity class, with its own set of relations. For example, simultaneously with learning the value of, say, a dime, one learns that it is the equivalent of two nickels. Knowing this fact induces a new kind of arithmetic strategy. As one adds a nickel + a nickel, one is more likely to know automatically that the answer is a dime than to go through the usual set-union arithmetic of 5 + 5. Asked "how much is five nickels?" not many people would bother multiplying 5 × 5; rather, they would know as a memorized monetary-arithmetic fact that the answer is a quarter (note: not 25¢). There is also a new strategy involved, that of rounding off; one comes to regard $4.95 as, for all intents and purposes, $5.

The tailors often work with currency in their heads. The arithmetic associated with this system is based on daily practice in handling money through transactions in the shop and elsewhere. Hence, operations are seen as manipulations of coins and bills or, rather, the concept associated with them, not as manipulations of numbers. For dollars and pennies there may be a perception that $3 + $4 or 3¢ + 4¢ is identifiable with the number problem 3 + 4, but whenever fractional parts other than pennies are involved, the monetary relationships take over and adding 5¢ and 5¢ is seen, not as 5 + 5, but rather as a nickel + a nickel. Hence the answer is a dime, translatable to 10¢, not 10¢ directly. Because we felt that computations involving fractions of dollars were learned, and hence perceived, differently from numbers, we predicted that the tailors doing Vai/Gola monetary arithmetic would find similarities between the problems $18 + $19 and 18¢ + 19¢ more readily than between those and $1.80 + $1.90, and that the first two, in fact, would be seen as more similar to 18 + 19 than to $1.80 + $1.90. Furthermore, given the tendency to round off, we anticipated answers like $4 to the problem $1.80 + $1.90.

As a result of these considerations, we expected different errors on the monetary problems than on the number problems for anyone who

was using Vai/Gola monetary arithmetic. We expected more answers off by one of the coins (nickel, dime, quarter, half-dollar), and we expected there to be a failure to recognize the same problem when the decimal point occurred in such a way as to introduce both dollars and fractions of them. By contrast, we reasoned, those who had learned to do monetary calculations at school would have been taught the mathematical identity of numbers and dollars, shown that the decimal was subject to certain strict manipulative considerations, and taught that if one kept those considerations in mind the arithmetic for money and the arithmetic for nonmonetary quantities were identical. Hence, we felt that the errors would be of the same sort as with discrete quantities and there would be no distinction as to difficulty between the problems 18 + 19, \$18 + \$19, 18¢ + 19¢, and \$1.80 + \$1.90.

CONSEQUENCES OF COMBINING SYSTEMS

As should be clear from these examples, the mode in which one expresses a number has decided, and sometimes unexpected, influence on how one deals with it arithmetically; and considerations of this sort led us to envision the possibility of errors that involved more than one arithmetic system. If a tailor had more than one arithmetic system at his disposal, we asked ourselves, what sorts of errors might he make? First, because to us money seemed so different from the more general, pure numbers, we considered the overlap of monetary and nonmonetary calculations. Here was a clear distinction between problem-solving domains. Observation and common sense suggested that seldom if ever would a real-life problem be reworded in terms of money (for example, "add a nickel of oranges and a dime of oranges" rather than "add five oranges and ten oranges"). Money is much more likely to be introduced/learned after pure quantitative numbers. Hence, if any switching at all is done, it is more likely to be "add five pennies and ten pennies" for "add a nickel and a dime." In reality, nickel and dime, being shorthand for more general concepts (5¢ and 10¢), are likely to be learned later and, should any switching occur, to be interpreted in terms of the earlier-learned concepts. Our conclusion was that we should expect a discontinuity in arithmetic strategies between quantitative and monetary systems for those who had not been specifically exposed to and had pointed out to them the analogues; those not in school would not have noticed these analogues, both because of the discontinuity in domains of problem solving and because of the formulation of the number names themselves.

Another way in which errors not related specifically to either one or another arithmetic system might be introduced would occur if there were a transition between one numeration system and another, disregarding problem domain and strategy. The fact that Vai and Gola are accustomed to tallying at 5, 10 and 20 should predispose them to recognize that the Western system has a similar effect at 10, particularly given the explicitness of the Gola version and the written Arabic. Therefore we projected a whole class of translation-analogic errors for native Vai/Gola speakers who were in the process of acquiring Western number

names. These we classified as codification and translation errors. For example, we anticipated that in the course of learning and translating the Western system one might sporadically replace the tallying point 10 with a 5 or a 20. This would lead to forms such as 11 for "a 5 and a 1" (hence 6) or "a 20 and a 1" (hence 21). There are, clearly, many such possibilities. There is also the reverse possibility of 11s being misread as 6s. An even more confused rendering might mistake 15 or 51 for "a 5 and a 1," that is, 6. Such errors, should they occur, would be particularly exciting, because they would indicate a transition stage and, more important, a recognition by the individual of as yet imprecise analogues between two quite distinct numeration systems.[7]

With all these ideas in hand, we can now demonstrate the way we used them to analyze the performance of groups and individuals.

GROUP AND INDIVIDUAL DIFFERENCES

We had three kinds of data to work with: 1) a table giving the errors of each tailor on each problem (in which the identities of the tailors were suppressed); 2) protocols taken during the administering of the test, in which the actions and explanations of the tailors were recorded in detail; and 3) interview data about the apprenticeship and schooling experiences of the tailors. We started with the error data, making tentative predictions about the arithmetic system being used when particular errors are made. Then we checked these predictions by examining the protocols and interview data. Rather than discuss the results of the analysis in detail (see Lave n.d.), we will demonstrate here how fine-grained differences in problem-solving processes may be delineated using our techniques.

We predicted that Tailor 217 was a stable user of Vai/Gola arithmetic and had probably not learned school arithmetic when we found him making the following errors: $6 \times 3 = 19$, $5 \times 5 = 24$, $18 \div 3 = 5$, $139 - 20 = 121$, $8 \times 3 = 25$, $64 - 8 = 54$, $8 \times 3 = 28$ (a second version of the problem), $41 - 8 = 32$. (Furthermore, he was unable to work 28×3.) His performance fit our conjectures that answers on the Vai/Gola system would be off by small amounts and that large numbers would present more than average difficulty.

By contrast, Tailor 075 made the following errors: $6 \times 3 = 12$, $23 + 8 = 32$, $8 \times 3 = 26$ (twice), $48 - 7 = 31$, $15 \div 5 = 4$, $7 \times 5 = 45$; he was unable to do 6×4. This appeared to be a person with some schooling, for we expected tailors using Western algorithms to make borrowing and carrying mistakes (and therefore to be off by 10 in addition and subtraction problems) and to misrecall arithmetic facts. Although some of his wrong answers might be grouping/counting errors, this seems unlikely in the light of the consistency of his value for 8×3. That he did $139 - 20$ correctly indicated a facility with large numbers that strengthened the argument.

In addition, he was able to handle correctly one of two problems involving numbers that are intrinsically awkward in Vai/Gola numeration: he successfully solved $65 - 9$. Because he replaced the multiplication by

addition in 40×5, we could not judge his ability to multiply 40; but, particularly because he was able to do other multiplications, his error indicated that he still found 40 a problem.

For these tailors, we found that the protocols confirmed that the first was using counting techniques and the second, school algorithms. Interview data showed that the first had not been to school and that the second had five years of schooling.

Next we examined the performance of tailors with apprenticeship but no schooling and tried to detect differences between those with little and those with a lot of experience as tailors. Again we started with the error chart and tried to predict which tailors would belong in which subcategory. We started with the problems $65 - 9$ and 40×5. Neither of them is particularly difficult in the school-taught arithmetic system, but both are difficult in the Vai/Gola. We reasoned that if a person were using Vai/Gola arithmetic but had had more practice with it, he would be likely to make fewer counting errors and more errors related to the numeration system itself. The two arithmetic problems gave us the opportunity to look for such errors, and we found them. As we have said, we expected a great deal of difficulty with multiplying 40; since it is already 2×20, 40×5 becomes something like, $2 \times 20 \times 5$. We found strong confirmation that tailors who worked in Vai/Gola and had difficulty frequently gave answers that indicated they were confusing 40 with 20. When we coupled this with $65 - 9$ (in Vai/Gola $[3 \times 20 + 5] - [5 + 4]$), for which Vai/Gola arithmetic could conceivably yield erroneous answers such as 51, 46, 41, or 36 (low respectively by 5, 10, 15, or 20), we felt we had a pretty good means of identifying those who were using Vai/Gola with facility—who had passed from simple counting to more advanced arithmetic. As a final piece of evidence, we expected that such people would also have a corpus of memorized arithmetic facts (those of repeated experience), such as small common multiples and the like.

Tailor 058 seemed to fall into this category, with the following errors: $12 \div 3 = 3$, $5 \times 5 = 20$, $18 \div 3 = 4$, $65 - 9 = 46$, $40 \times 5 = 180$, $139 - 20 = 118$, and a large number of what are clearly counting errors. When we looked at the interview data, we found that Tailor 058 was a master tailor with no schooling. We found in this subcategory of thirteen tailors, seven who gave answers to the $65 - 9$ problem that we felt reflected Vai/Gola arithmetic thinking. (All but one of the remaining six solved the problem correctly.) Further, every one of the six errors for 40×5 made by this group seemed to us to reflect the nature of 20 in the Vai/Gola numeration system. Not only that, but we found that Vai/Gola-type errors on $65 - 9$ and 40×5 occurred among those with little or no schooling; but not among those with more schooling, which strengthened our belief in our identification of the arithmetic system being used.

As an example of an alternative check on the amount of tailoring experience of those with no schooling, we considered monetary calculations. It seemed likely that the apprentices, those tailors having less shop experience, would do less well on monetary problems than the masters. Furthermore, we felt that an even finer distinction could be made by looking at how completely the masters had been able to recognize the similarity

between identical problems that were couched in both monetary and nonmonetary terms. We were especially interested in problems that involved both fractional and whole parts of dollars; this meant ones that might involve division and ones like \$2.30 + \$.80.

For both masters and apprentices with no schooling, it turned out that there were errors such as \$60 ÷ 10 = \$5.50, \$18 ÷ 3 = \$8.65, \$2.30 + .80 = \$3.35, and so on. Answers of this sort did not occur for tailors with one or more years of school. Answers for the addition problem were off (with one single exception) by amounts readily ascribable to base 10 arithmetic, whether written or mental, and not Vai/Gola monetary arithmetic.

In addition, whereas no master without schooling said he could not work the problem \$2.30 + .80, three out of twelve apprentices did, and two more gave idiosyncratic answers similarly on most of the other monetary problems; the apprentices gave numerous "I don't know" responses, while the masters gave almost none. Of particular interest was the problem \$139 − \$20. None of the apprentices with no schooling who missed it gave counting-like errors, and only one of the wrong answers was at all reasonable. This indicated not only that monetary amounts, particularly large ones, were strange, but that the apprentices, most of whom had been able by counting to solve 139 − 20 roughly as a numerical quantity, did not recognize that they could do so for dollars as well. Hence we felt we had a sound way by which to distinguish between experienced and inexperienced tailors, using monetary arithmetic. Further, we could make a fairly clear distinction between those with no schooling and those with a little schooling by looking at the same problems.

It was also possible to look at the performances of individuals by much the same means. Making conjectures based on abstract arithmetic properties, looking for diagnostic errors, and then checking for verification of the conjectures by looking at protocols and interview data provided proof of the consistency of individual tailors' performances. It also pointed to instances in which an individual was shifting from one arithmetic system to another, in either numeration system, strategies, or both. For example, Tailor 035 made two kinds of errors; some looked like Vai/Gola counting errors (12 ÷ 3 = 3, 28 + 3 = 32, 60 ÷ 10 = 4, 28 + 5 = 100) and some looked like base 10 arithmetic errors (139 − 20 = 109, 7 × 3 = 31, 39 − 8 = 21). That anyone should be changing arithmetics was, at first, a hypothesis, but looking at the protocols proved that such switching actually did occur. For example, we found tailor 231 using marks and tallies as well as school-taught algorithms for addition and subtraction. It appeared that, given the stable use of the school-taught arithmetic system by those with five or more years of school, the tailors who were attending school were in fact switching from the Vai/Gola system to the school-taught system as their customary system of choice.

Further evidence for system switching was found in the codification errors we have mentioned. We found such answers as 11, 15, or 51 for the translation of the Vai/Gola word for 6, in response to the problem 60 ÷ 10, primarily among individuals with one to four years of schooling.

This same subsample showed a mix of Vai/Gola and school errors on 65 − 9 and 139 − 20.

Other sets of switchings, found in the protocols, involved applying Vai/Gola strategies—often in combination—to the school-taught numeration system. For example, Tailor 214 rewrote the problem 6 × 3 as (3 + 3) + (3 + 3) + (3 + 3) = 6 + 6 + 6 = 18. When methods like these occurred, they represented examples of an incorporation of the two systems into a new, more flexible and more effective arithmetic system. Thus, we were not only able to detect what the tailors were doing because of their past learning experiences, but we were also able to see instances of the synthesizing of two disparate systems to create a third. This new system has to a large extent shifted to the school-taught numeration system; it combines some of the standard school-taught algorithmic strategies with the Vai/Gola techniques of breaking down numbers into smaller, more easily managed ones, grouping them, and tallying. This is a fascinating subject in its own right and will be discussed at length elsewhere. What is important here is that the techniques of analysis we used enabled us to see this change in progress and to be able with a great deal of accuracy to predict which of the tailors were engaged in it.

CONCLUSIONS

Three questions should be raised in summing up and assessing the work presented here: what have we learned about the tailors' use of arithmetic in their daily lives? How did each of the various methods of data gathering and analysis contribute to our understanding? To what extent does the domain of arithmetic contribute to the description of behavior that is crucial for comparative cognitive research?

We have learned the details of several arithmetic systems. To do so, we have had to become familiar with the several numeration systems of the tailors and with their associated problem-solving strategies. We have discovered that the word "system" is appropriate in a social as well as a formal mathematical sense, for tailors with similar educational experiences do arithmetic in very similar ways. We believe it is valid to say that there is a system for doing arithmetic that is characteristic of Vai and Gola problem solving and is used by tailors who are trained by traditional apprenticeship methods and have not been to school; that there is another system that is used consistently by those with five or more years of schooling; and that individuals with a little schooling are in the process of switching from one system to the other. We feel we understand a good deal about the changes that occur in arithmetic competence as more educational experience is acquired through apprenticeship or school. To say that we understand several arithmetic systems in use also implies that we can guess what problems the tailors will have memorized answers to, how this compares with the memorized-fact category for school-taught tailors, what sorts of problems are within the everyday competence of tailors with different kinds of experience, and what sorts of problems are and are not encountered as part of their daily activities.

If the whole of the research on arithmetic is viewed strictly as an ethnographic enterprise, we conclude that it has been an extraordinarily fruitful one in helping us to understand the complexities of arithmetic practice in this multieducational setting.

The initial observation and informal participation in the tailor shops had several helpful results. It helped to establish the types of arithmetic skills that tailors need in their work. It helped to establish the fact that the learning of these skills does not take place at a determinant stage of apprenticeship. We learned what measurements and related operations are frequent in the work lives of the tailors, and what arithmetic games the tailors play in which the rules are similar to the rules we wanted to incorporate into the experimental tasks. Both participant-observation and the detailed interviews contributed to our knowledge of the individuals involved and their relevant idiosyncrasies. What we could not learn from participant-observation was how the tailors actually went about solving problems. Particular problem-solving processes are not casually observable; to draw inferences about them requires something like formal experiments. The participant-observation work made it possible to design experimental tasks in relation to the tailor's inventory of practical mathematical knowledge and to anlayze the results in terms of many background and educational factors about the individual tailors.

Without the experimental work, particularly the error analysis and the protocols describing both problem-solving behavior and verbal explanations of that behavior, we would not have come to understand the tailor's arithmetic systems. Our inferences about the nature of problem-solving processes depended heavily on being able to compare problems and solutions whose systematic interrelations made it possible to confirm or eliminate competing hypotheses about what problem-solving processes were being used; they also required that many individuals do the same problems under very similar circumstances. Analysis of the error data and protocols sensitized us to problems and difficulties the tailors faced in carrying out arithmetic in the test situation and sent us back to the tailor shop for observation and more informal work on the arithmetic hurdles of daily life, which focused on some particular problems about which we had not thought to be observant before.

In trying to indicate the separate advantages of the broad range of research methods, we have fallen into a description of its diachronic aspects. We moved from observation to some experimental tasks that were relatively close to the everyday scene. From there, we went to more formal experimental tasks (reported here) and then back to observation, armed with hypotheses about what we ought to see if our experimental results were adequate descriptions of everyday arithmetic activities. It may well be that rapprochement between naturalistic and experimental data gathering and interpretive activities is more a product of the number of iterations of the two kinds of methods, than it is of the particular ways in which we have applied them on any given occasion.

The importance of the domain of arithmetic to cognitive research is derived partly from its formal properties. Using its enormously refined notation system, we can describe differences in problem-solving activities

as small as the differences between $(3 + 3) + 6 + (2 \times 3) = 18$ and $6 + 6 + (2 \times 3) = 18$ and $(3 + 3) + (3 \times 2) + 6 = 18$. Important as it is as a descriptive tool, arithmetic is even more helpful in the study of cognitive processes because of its systematic nature. The mathematician can predict what procedures will be applied to a given problem, with what variants, and with what results.

We have said that a given arithmetic system can be viewed as evolving, and we have described four different arithmetic systems in Vai/Gola tailors' culture. In drawing inferences about problem-solving processes, then, we started with the systematic properties of the "folk" arithmetic system, not a set of general properties of all possible arithmetics. It is important to recognize that folk arithmetic systems are quite systematic enough to have the powerful characteristics we have attributed to the abstract domain of the Western mathematician. This helps to account for the fact that folk practitioners of arithmetic are, in our experience, far better able to articulate the steps they go through in working out an arithmetic problem than to report the steps involved in other familiar processes (for example, the process of sewing the waistband onto a pair of trousers).

The "high-resolution" picture of individual differences that resulted from studying arithmetic has made it possible to demonstrate consistent differences in the methods employed by tailors who have learned arithmetic in shop and school and among tailors with different amounts of shop experience. Problem-solving procedures are closely related to educational experiences in shop and school. We have good evidence that all but the youngest and least experienced apprentices have a clear understanding of general principles of arithmetic problem solving. Errors, for all the tailors, are sensible: they stem from the arithmetic system in use.

Thus, the data do not support speculations by some psychologists and anthropologists that a skill learned incidentally in the course of daily activities necessarily leads to less general understanding than one learned "from the top down" in school. Rather, our data suggest that the practice opportunities available in the learning environment, and the social costs associated with various types of errors, account much better for the difficulties encountered by tailors with different educational experiences. Thus, the tailors who learn arithmetic in the shop setting double and halve with alacrity; rarely do they encounter three-digit numbers, or even large two-digit numbers in the shop, however. They do have difficulty on the math test with large-number problems. Tailors who have been to school and have switched to school-taught arithmetic apply algorithms to problems of all sizes with greater ease but are more often the authors of wild errors that are left unnoticed. In the tailor shop, a very large error in trouser measurements or a customer's change would have more serious social consequences for the tailor than a very small error; but there is no different penalty for large errors than for small ones in school. The tailors' performances reflect these differences in their daily uses of arithmetic. We would guess that the same principles may apply to a variety of other cognitive functions: the organization of prac-

tice, rather than early instructional circumstances, may account for much of the variance in solution strategies.

The arithmetic data also provide generalizable lessons about how people manage distinct systems for solving closely related problems. It appears that the organization of arithmetic tasks into separate domains (money and discrete quantities) is a major factor supporting individuals' ability to keep two separate arithmetic systems mentally available. We do not find two systems comfortably coexisting for dealing with a single domain of problems, however. Instead, discrete quantity arithmetic learned in school *replaces*, through a rather gradual transition, discrete quantity arithmetic learned in the shop. It may be generally true that the separation or merging of problems in the social/physical environment has a major impact on the organization of cognitive skills in peoples' heads. It is just such culture/cognition interrelations that deserve collaborative attention from psychologists and anthropologists in the future.

Multiple arithmetic systems in a single cultural setting are probably the rule rather than the exception. Reasons for the development of multiple arithmetic systems among the tailors include multilingualism, often leading to the coexistence of several numeration systems. Also, where both spoken and written versions of arithmetic systems exist, they often differ. People who are exposed to more than one educational tradition are likely to have more than one arithmetic system. There are different arithmetic systems in compartmentalized problem-solving domains: one of the most common forms of segregated problem domains may be the coexistence of monetary and nonmonetary arithmetic systems. Given that all these possibilities exist for a large part of the world, it is likely that the techniques of analysis used in the Liberian tailor studies would have application elsewhere as well.

NOTES

1. We presume that similar considerations have helped to produce the series of works on arithmetic skills by a number of well-known psychologists (Thorndike 1922; Bruner 1964; Ginsburg 1977; Resnick 1976).
2. Reed is a mathematician, an algebraist who also has strong interests in the linguistic features of arithmetic systems. Lave is an anthropologist who has done research on the impact of apprenticeship on the cognitive skills of tribal tailors in West Africa. The field research on which this paper is based was carried out in Liberia between 1973 and 1978. It was supported by research grants from the Office of Education, the Foundation for Child Development, the National Institutes of Health, and the Spencer Foundation. The analysis of the arithmetic data received support from the Institute for Comparative Human Development, the Rockefeller University. We wish to thank all these institutions for their support. Colleagues who have contributed to our thinking about the issues discussed here include Michael Cole, Hugh Gladwin, and Sylvia Scribner. We thank them for sharing their time and thoughts.
3. Through observation and participation in the activities of the tailor shop, we found that tailors must be able to read off inches on the tape measure; measure various lengths; and quarter, halve, double, and quadruple measurments as they cut out the four main pieces of a pair of trousers before assembling them

into a finished product. Multiplying and dividing by 2 and 4 are therefore by far the most common operations. Tailors must be familiar with some units of measurement that are not commonly used by nontailors, namely, the yard, the half-yard, and the inch. They make trousers in many different sizes, but a few sizes are made much more frequently than others; thus some numbers on the tape are used more often than others, and some of these are more likely to be doubled or halved. In going from the customer measurement phase to the finished product, precision within an inch or even two inches is considered adequate; consequently, tailors rarely use fractions.

4. It is possible to write out the number names in the Vai syllabic script, but this is not done. Instead, both English and Arabic scripts are used in writing numbers in Vai script manuscripts. There are some slight differences between Vai and Gola numeration, but these are not significant for the discussion.
5. Her book contains many examples of such systems in West Africa. Menninger (1969) discusses abstract systems and examples from many regions.
6. Zero, used as a place holder, indicates that there are no units in a particular column (or place). Thus in the number 703, the zero indicates that there are no units of size 10 in the number. This contrasts with the other use of zero, to represent the number that comes before 1, usually glossed as "none."
7. Another set of errors, we felt, had less to do with the arithmetic system than with the incorrect identification of the operation required by the given problem. We expected most of these errors to occur for division and multiplication, the most difficult and latest learned of the four arithmetic operations. Also, we expected that there would be two kinds of errors: the replacement of an operation by its inverse (multiplication for division, addition for subtraction, and so on) and the replacement of an operation by some operation other than its inverse, especially addition for multiplication.

   Of course, the tailors were allowed to respond "I don't know how to do this." When such a response occurred, it was sometimes possible to get the test taker to attempt the problem; on occasion, he did. If a tailor really could not do the problem, there were several possible causes. One cause might be that he could not abstract the features and data required by the problem from the embedding story problem. Again, he might be unfamiliar with the operation required, or find the numbers involved so large or awkward that he could not handle them. Or he might know the right operation, but be unable to carry out the strategy. Hence all the factors—problem domain, numeration system, and strategy—could be involved in producing an "I don't know" answer.

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22

Memory and the Assessment of Behavior

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In the social sciences people have been used as measuring instruments for the investigation of many aspects of behavior. To find out if the subjects in an experiment are *acting aggressively*, for example, an investigator will typically use other people as measuring instruments. On the basis of their understanding of what constitutes acting aggressively, the observers will judge whether various segments of the subjects' behavior are *aggressive* or not. Attempting to specify what *aggressive* means by defining the term with reference to more specific acts, such as *hitting* or *insulting*, still fails to make the decision process explicit, since the rules for coding behavior remain dependent on a set of undefined terms. Unless the measurement process is based solely on physical properties, at some point such verbal coding rules always fall back on undefined terms.

The advantages of methods using human judgment as a means of measurement are generally thought to outweigh the disadvantages. A few social scientists require that all their measurements be mechanically recorded in units of mass, time, and distance, but if the behavior to be recorded is even slightly complex, it becomes enormously difficult to attempt such a mechanization of the measuring process. Primarily, this difficulty involves the lack of one-to-one correspondences between the properties of physical signals and the distinctions perceived by human observers. For example, in the area of speech at least three quite different acoustic signals are heard as initial *d*, depending on the following vowel (Liberman et al. 1967).

These many-to-one and one-to-many types of correspondence, which make mechanical decoding of complex human behavior inefficient and costly, occur between many levels besides the physical medium and the perceptual response. For example, there are many ways to indicate *agreement* besides the sentence "I agree." There are alternatives in vocabulary and syntax, and in the use of a medium such as speech rather than gesture or writing. A mechanical device which could determine as well as a human judge when a respondent agreed with a statement, whatever the means of agreement used, would be a most amazing and complex piece of machinery. At present, it is much easier to use humans than to try to build behaviorscopes from wire and glass.

THE VALIDITY AND RELIABILITY OF HUMAN BEHAVIORSCOPES

The low cost of using human judges to assess attributes of human behavior is offset by a number of disadvantages. Traditionally, these disadvantages

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† Source: Measurement in the Social Sciences. (Chicago: Lieber-Atherton, 1974).

have been discussed as problems of *reliability* and *validity*. Reliability is usually thought of as the degree to which measuring instruments of the same type give the same results. A large literature has developed concerning the causes, effects, and remedies for deficiencies in reliability. In general, present-day social science shows a high degree of sensitivity to problems of reliability.

In contrast to problems of reliability, problems of validity are not at present well formulated. In a classic article, Campbell and Fiske (1959) presented one of the first methodological assessments of validity. They argue that validity, like reliability, involves agreement between measures. Validity, however, involves agreement between maximally different, or independent, measurement procedures; whereas reliability involves agreement between maximally similar measures. Thus two different methods producing very divergent results cannot both be measures of the same thing, although both might be valid measures of different things. From examples in Campbell and Fiske's article, it appears that very few of the social sciences' methods show this type of validity in measuring any aspect of human behavior.

Obviously, high reliability alone does not insure validity. It is easy to show that under certain conditions highly reliable judgments can be quite invalid. The Muller-Lyer optical illusion, for instance, provides a simple example of conditions which elicit reliable but invalid judgments about the lengths of two lines. The clinical illusion, in which clinicians reliably interpret projective tests with invalid predictions, is a similar phenomenon, but it is cognitive rather than perceptual (Chapman 1967).

This chapter attempts to demonstrate that one of the methods used in the social sciences is invalid because it is subject to a special effect analogous to that of an illusion; an effect in which there is a reliable and systematic distortion of judgments under certain conditions. The general procedure in which this method is often used, and in which the distortion biases the results the most drastically, can be described as follows: First, one or more human observers are asked to judge one or more subjects on a number of traits of behavior; the judgments are expressed in ratings or rankings (scores) based on the observers' long-term (i.e., more than ten minutes') memory of the subjects' behavior. Second, a single score is computed for each subject for each trait, usually by taking the mean of all the scores given to each subject for each trait. Third, to find out how the traits are related to each other, some measure of association, such as the product-moment correlation, is computed from the subjects' scores for all pairs of traits. Finally, the measures of association are analyzed to determine how the traits are organized with respect to each other. A number of kinds of analysis may be used, ranging from simple clustering by inspection to factor analysis or multidimensional scaling. The results of such analyses indicate which traits tend to go together, and the similarity structure of the trait measurements is taken as a representation of the structure or organization of the subjects' behavior.

The motive for using this procedure is usually to simplify the description of human behavior by grouping similar traits into multibehavior

units, such as clusters or dimensions. Since there appears to be a wide variety of human behaviors, each of which can be described in a large number of ways, some procedure is needed for constructing economical descriptions of a person's or a group's behavior. The reason for using ratings or rankings based on long-term memory in this procedure appears to be that such judgments are easy to obtain. One can consider the memory of each observer to be a storehouse, where the impressions left by hundreds of thousands of the subject's past acts may be inexpensively and quickly recovered.

Unfortunately, it appears that an observer's memory cannot be considered trustworthy for many kinds of judgments. The evidence to be presented below indicates that in such judgments there is a systematic distortion, in that traits the observer considers similar will be recalled as applying to the same person, even when this is not the case. As a result of this effect, the correlations found between traits prove to be due more to the observer's conception of "what is like what" than to covariation in the behavior of the subjects.

In an earlier paper on this topic it was argued that procedures which try to classify behavior from data consisting of global judgments by human observers reveal which behavior terms are semantically similar, not which behaviors of the subjects go together (D'Andrade 1965). The evidence used to support this contention was a demonstration that in at least two cases judgments of the semantic similarity of trait terms gave approximately the same results as the analysis or correlations based on the ratings of subjects' behavior made by informed observers. This evidence was equivocal, however, since it can be argued that the semantic similarity of trait terms corresponds to the way these traits actually go together, perhaps because the actual relations between traits become semantically coded into the trait terms. Given this *isomorphism* hypothesis, the fact that the same type of similarity structure can be obtained both from judgments of semantic similarity and from observer ratings would be expectable, and it certainly would not invalidate psychological theories which assume that people can be accurately described in terms of multibehavior units of some kind.

MULTITRAIT-MULTIMETHOD MATRIX CORRESPONDENCE TECHNIQUE

In order to decide between the *isomorphism* hypothesis and the *systematic distortion* hypothesis, a comparison between ratings based on long-term memory and the actual behavior of the subjects is needed. If the observer's memory-based ratings showed a very different pattern of correlations from that found for the data based on the actual behavior of the subjects (but a pattern similar to judgments of semantic similarity), it would be reasonable to reject the isomorphism hypothesis and to consider the systematic-distortion hypothesis supported. This strategy is similar to Campbell and Fiske's multitrait-multimethod matrix technique, except that entire patterns of correlation coefficients, instead of specific correlation coefficients, are to be compared. Pattern comparison is needed

here because it is not the validity of specific traits or categories which is in question, but the validity of the correlations found between traits.

Ideally, for recording the actual behavior of the subjects, a mechanical device should be constructed to count frequencies of different kinds of behavior. Unfortunately, for all the reasons discussed above, a mechanical measuring instrument is not practical at present for any judgment more complex than *is making noise* versus *is not making noise.* The closest approximation to a mechanical device appears to be a trained observer using a simple coding scheme to record a subject's behavior as it occurs. The immediacy of the observer's assessment and the simplicity of the coding decisions should, it is hoped, protect against systematic distortion of the type thought to take place in long-term memory.

The most frequently used coding scheme for recording ongoing behavior is the Bales *Interaction Process* category system. The Bales categories are relatively simple in terms of coding rules and are also usable for ratings or rankings based on long-term memory. A schematic classification of the Bales system is presented in Figure 1.

The Borgatta, Cottrell, and Mann Study

Because of the general applicability to different methods of measurement of the Bales category system, a search was made to find published studies

A: Positive Reactions	1. Shows solidarity, raises others' status, jokes, gives help, reward
	2. Shows tension release, shows satisfaction, laughs
	3. Agrees, shows passive acceptance, understands, concurs, complies
B: Problem Solving Attempts	4. Gives suggestion, direction, implying autonomy for other
	5. Gives opinion, evaluation, analysis, expresses feeling, wish
	6. Gives orientation, information, repeats, clarifies, confirms
C: Questions	7. Asks for orientation, information, repetition, confirmation
	8. Asks for opinion, evaluation, analysis expression of feeling
	9. Asks for suggestion, direction, possible ways of action
D: Negative Reactions	10. Disagrees, shows passive rejection, formality, withhold help
	11. Shows tension increase, asks for help, withdraws "Out of Field"
	12. Shows antagonism, deflates others' status, defends or asserts self

Figure 1. Bales interaction-process analysis categories (taken from Parsons and Bales 1955:267).

using the Bales categories in which both immediate recording of ongoing behavior and judgments based on memory had been carried out. Two such studies were found in which the data were reported completely enough to permit the necessary multitrait-multimethod matrix comparisons. The first study was conducted by Borgatta, Cottrell, and Mann (1958); the second was conducted by Richard Mann and reported in his Ph.D. dissertation (1959).

As part of the Borgatta, Cottrell, and Mann study a number of small groups were observed directly, with immediate recording of ongoing behavior according to the Bales category system. After twenty hours of small-group contact the individuals in each group also ranked each other on a variety of personality traits, including a set of behavior descriptions which correspond approximately to the Bales category system.

The subjects in the study were forty-seven graduate students enrolled in a class on interpersonal relations. They were divided into five small groups, each meeting for a two-hour discussion period every week throughout the semester. The groups had little or no external supervision. The primary focus of discussion was the analysis of processes that go with "democratic leadership."

The questionnaire ranking data were collected by the course instructor after the ninth week, ostensibly as part of a "student evaluation procedure." Rankings were made on a total of forty traits, with each person ranking all members of his group. Since the groups were of slightly different sizes, rank scores for each group were equalized. The traits most closely approximating the Bales categories were:

1b. Shows solidarity and friendliness (Bales no. 1)
2b. Is responsive to laughter (Bales no. 2)
4b. Makes the most suggestions (Bales no. 4)
10b. Disagrees most (Bales no. 10)
11b. Tends to be nervous (Bales no. 11)
12b. Tends to be antagonistic (Bales no. 12)

Presumably the investigators felt that their rephrasings of the category labels made it easier for the subjects to rate each other.

The immediate recording of ongoing interaction was carried out by a trained observer using the Bales category system. Each of the five groups was observed during the ninth and tenth weekly sessions. Interaction scores were adjusted to the individuals' rate of initiated acts per 100 minutes. Generally the groups show slightly different and stable interaction profiles.

To relate trait rankings to the results of the immediate recording of ongoing behavior, product-moment correlations were computed between all variables. These correlation coefficients indicate the degree to which individuals who have high scores on one variable also have high scores on the other variable. From the complete matrix of correlations it is possible to compare the pattern of correlations found between Bales category scores for rates of behavior based on immediate recording and the pattern of correlations found between Bales category scores for ranking based on long-term memory. Borgatta and his associates did not obtain

measures of semantic similarity, however; so—to make possible a comparison of the pattern of semantic similarity occurring between the Bales categories and the patterns of correlations found for the other types of data—a test of semantic similarity had to be constructed and administered.

Because the wording of the trait descriptions used by Borgatta, Cottrell, and Mann is slightly different from that used in the original Bales category system, a modified set of behavior descriptions was constructed for the test of semantic similarity which is somewhat closer to Bales's wording. The phrases used were:

1s. Shows solidarity (Bales no. 1)
2s. Jokes, laughs (Bales no. 2)
4s. Suggests, gives direction (Bales no. 4)
10s. Disagrees (Bales no. 10)
11s. Shows tension, nervous (Bales no. 11)
12s. Shows antagonism (Bales no. 12)

The test was administered in questionnaire form to ten graduate students, none of whom were previously acquainted with the Bales category system. The format used for this questionnaire is presented in Figure 2.

Table 1 presents the correlation matrices from Borgatta, Cottrell, and Mann for the relevant Bales categories for rates of behavior based on immediate recording, for rankings based on long-term memory, and for the mean ratings of semantic similarity (ranging potentially from −3.0 for "very dissimilar" to +3.0 for "very similar").

For comparing matrices, the agreement in rank order for corresponding coefficients was used as a measure of overall pattern similarity. Degree of agreement in rank order is given by the Spearman rank-order correlation coefficients, which indicate how well the degree of similarity found between a given pair of Bales categories for one method of measurement predicts the degree of similarity to be found between the same pair of categories for a second method of measurement. Thus the coefficients inside a matrix indicate how much alike the Bales categories are for a particular method of measurement, while the rank-order coefficients between matrices indicate the degree to which different methods of measurement yield similar patterns of association between Bales categories.

The results presented in Table 1 may be summarized as follows:

1. Correlation coefficients for behavior rates based on immediate recording tend to be smaller in absolute size than the correlation coefficients for rank judgments based on long-term memory.
2. The correlation matrix for behavior rates is not strongly similar to the correlation matrix for rank judgments ($r_s = .34$).
3. The matrix of semantic-similarity ratings is fairly similar to the correlation matrix for rank judgments ($r_s = .60$), but not similar to the correlation matrix for behavior rates ($r_s = .03$).

Generally, information revealing which Bales categories are most alike when behavior rates based on immediate recordings are used will not predict very well which Bales categories are most alike when rank judg-

Judgment of Behavioral Descriptions

The purpose of this questionnaire is to obtain your judgment about the degree of *similarity* between a series of descriptions of human behavior. Base your judgments of similarity on the meanings of the terms and the degree to which the descriptions typically refer to the same kinds of behavior.

Each question on the test consists of a pair of behavior descriptions to be judged on a seven point scale ranging from *'very similar'* to *'very dissimilar.'*

In the example below, please rate the descriptive terms *'cooperative'* and *'helpful'* according to your estimate of their degree of similarity.

cooperative : : helpful

very similar	generally similar	slightly similar	unrelated not similar not dissimilar	slightly dissimilar	generally dissmiliar	very dissim-ilar
+3	+2	+1	0	−1	−2	−3

Place an *X* in the slot which corresponds most closely to your judgment of how similar the descriptive term *'cooperative'* is to the term *'helpful.'*

Now go on to the rest of the test. Please answer all the questions.

shows solidarity : : suggests, gives direction

very similar	generally similar	slightly similar	unrelated not similar not dissimilar	slightly dissimilar	generally dissimilar	very dissim-ilar
+3	+2	+1	0	−1	−2	−3

FIGURE 2

ments based on long-term memory are used. The pattern of association for the rank judgments for the Bales categories resembles the pattern of semantic similarity for these categories more than it resembles the pattern of association found for the observed rates of these behaviors.

These results appear to support the hypothesis that people tend to recall traits considered similar as characteristic of the same individuals. The evidence from the behavior rates based on immediate recording does not support the isomorphism hypothesis, since the categories considered similar in meaning are not the categories showing strongly correlated behavior rates.

What seems to happen in this type of small-group situation is that, while group members agree to a certain extent in their assessments, a number of these assessments are based, not on fact, but on a combination of other assessments. For example, according to the behavior rate correlations, individuals who *show solidarity* are slightly more likely than not to also *show antagonism* ($r = .17$). But since people conceive of solidarity

TABLE 1. Comparison of Correlation Matrices for Behavior Rates Based on Immediate Recordings, Rank Judgments Based on Long-Term Memory, and Semantic Similarity Ratings, All Using Bales Interaction Process Analysis Categories.

Correlations for

Behavior Rates

		1a	2a	4a	10a	11a	12a
solidarity	1a		.35	.35	.06	.09	.17
jokes	2a			.22	.08	−.11	−.13
suggests	4a				.17	.04	−.06
disagrees	10a					.08	.13
tension	11a						.05
antagonism	12a						

Rank Judgments

		1b	2b	4b	10b	11b	12b
solidarity	1b		.32	.24	−.33	−.22	−.50
jokes	2b			.34	−.04	.08	−.04
suggests	4b				.35	.00	.17
disagrees	10b					.35	.75
tension	11b						.28
antagonism	12b						

$r_s = .34$

$r_s = .03$

$r_s = .60$

Semantic Similarity Ratings

		1s	2s	4s	10s	11s	12s
solidarity	1s		.0	.2	−.7	−1.4	−1.5
jokes	2s			.1	−.9	.3	−1.0
suggests	4s				−1.1	−1.4	−1.1
disagrees	10s					.3	.7
tension	11s						1.0
antagonism	12s						

NOTE: r_s coefficients are Spearman's rank order correlation measure.

TABLE 2. Correlations between Behavior-Rate Category Scores and Rank-Judgment Category Scores.

Behavior Rate Category		Rank-judgment Category 1b	2b	4b	10b	11b	12b
Solidarity	1a	.23	.48	.36	.31	.03	.22
Jokes	2a	.00	.17	.14	.17	.03	.17
Suggestion	4a	.08	.17	.38	.09	.06	.00
Disagrees	10a	−.03	.05	.11	.30	.11	.22
Tension	11a	.07	.00	.29	.16	.29	.00
Antagonism	12a	.17	.43	.20	.08	.06	.14

NOTE: Correlations $\geqslant \pm.24$ significant at $p \leqslant .05$ for $n = 47$.

as very dissimilar to antagonism (semantic-similarity rating of −1.5), they will remember an individual who shows a good deal of *solidarity* in his behavior as *not* showing *antagonism* (rank judgment based on long-term memory $r = -.50$).

Further information about the relation between the behavior rates and the rank judgments can be obtained from an inspection of the correlations between these two sets of scores. These correlations, presented in Table 2, are based on the scores assigned to individual subjects; a high correlation coefficient between a particular behavior-rate category and a particular rank-judgment category means that, if an individual's score on one measure is given, a fairly good prediction can be made of that individual's score on the other measure. In Campbell and Fiske's terminology, Table 2 presents a *hetromethod* correlation matrix, and the correlations between categories with the same label are validity coefficients.

While the underlined validity coefficients average slightly higher than the hetromethod-hetrotrait coefficients, the sizes of these coefficients are not impressive.

THE RICHARD MANN STUDY

The complex pattern of correlations found in Table 2 raises a possibility which might explain the lack of correspondence between the behavior-rate correlations and the rank-judgment correlations. This is the possibility that the Bales categories may mean something different to the subjects, who make the rank judgments, than to the observer, who does the coding of ongoing behavior. A related possibility is that behavior-rate scores may be overly influenced by the general activity level of the individual; for example, a person who says very little, but whose statements often contain suggestions, may receive a lower rate score on *suggests* than an individual who performs a much wider variety of behaviors but is extremely active. In contrast, the ranking judgments may more nearly reflect the proportions of a person's behavior which fall in the various categories.

Controlling for these possibilities requires a study in which the category scores for the immediate recording of behavior are given in terms of

proportions of the individual's total output of behavior, and in which the observer not only does the immediate recording of behavior but also makes memory-based judgments about what proportion of each person's behavior falls into the various categories.

A study using both these procedures was carried out by Richard Mann (1959). The study has an extra feature, in that the small-group sessions took place under two different experimental conditions, with all subjects taking part in both types. Mann's research is an excellent example of comprehensive and careful data reportage. It includes test and performance scores for every subject on all variables, complete intercorrelation matrices, and examples of the test forms and instructions.

The major purpose of Mann's study was to test the relationship between personality and small-group performance. For purposes of this chapter, it is the group-performance measures which are of interest. These include the use of a modified Bales category system for the immediate recording of ongoing interaction. The same categories were also used for memory-based ratings, made after the small-group sessions by both the small-group participants and the observer.

For this study 100 male undergraduates, all of whom had recently pledged fraternities, were enlisted as subjects. After taking the questionnaire tests each subject was assigned to two different five-man groups in which none of the participants were well known to each other. No two subjects were put into the same two groups. One of the groups in which each subject participated worked on a relatively specific task (the "mined-road" problem) for fifty minutes, with the promise of a substantial reward if it came up with the best solution. The other group for each subject worked for fifty minutes on a more emotional and diffuse problem concerning "the way houses should handle pledge training." First, the subjects attempted to formulate a compromise group policy, after which they ranked the five fraternities represented by the group members in terms of their closeness to this ideal policy. Experimental conditions were balanced for order, with half the subjects beginning with the "task" condition, and half with the "social-emotional" condition.

Group sessions were held in a social science laboratory and were observed through a one-way mirror. After each fifty-minute session a post-meeting questionnaire was given to the subjects, who sat at separate tables; the observer also rated each group member.

The interaction during the fifty-minute session was scored by means of a modified version of the Bales category system. Each act was scored in only one category. The modified system is presented below:

Modified Categories (Bales-Mann)	Bales Categories
(1) Agreement and solidarity	*Shows solidarity* (no. 1) except for omission of all joking behavior plus *shows agreement* (no. 3)
(2) Laughing and joking	*Shows tension release* (no. 2) plus all joking behavior

Modified Categories (Bales-Mann)	Bales Categories
(3) Suggestions	*Gives suggestions* (no. 4)
(4) Opinions and orientations	*Gives opinions* (no. 5) and *gives orientation* (no. 6)
(5) Questions	*Asks for orientations* (no. 7) *Asks for opinions* (no. 8) *Asks for suggestions* (no. 9)
(6) Disagreement and antagonism	*Shows disagreement* (no. 10) *Shows antagonism* (no. 12)
(7) Tension	*Shows tension* (no. 11)

By grouping the Bales categories into fewer classes Mann hoped to make a simpler and more reliable coding scheme and to achieve a more normal distribution of categories. The observer, who had previously worked with Bales, achieved reliabilities of approximately .90 on the majority of the categories.

The scores for the immediate-recording data were transformed into percentages by dividing the subject's act frequencies for each of the Bales-Mann categories by the subject's total number of acts.

The questionnaire administered to the subjects after each session contained sixteen rating measures. Each of the five subjects rated the other four on these measures, using a ten-point rating scale with no ties permitted. The questions for the ratings on the Bales-Mann categories are:

1. (Agreement) Regardless of how much he talked altogether, how would you rate each member of this group (excluding yourself) on how much he tended to agree with what others had said?
2. (Laughing and Joking) Regardless of how much he talked altogether, how would you rate each member of this group (excluding yourself) on how much he tended to laugh and joke around?
3. (Suggestions) Regardless of how much he talked altogether, how would you rate each member of this group (excluding yourself) on how much he tended to give suggestions about what the group should do next or what decisions should be made?
4. (Opinions and Orientations) Regardless of how much he talked altogether, how would you rate each member of this group (excluding yourself) on how much he tended to give his opinion or state the facts about things?
5. (Questions) Regardless of how much he talked altogether, how would you rate each member of this group (excluding yourself) on how much he tended to ask questions of other people?
6. (Disagreement) Regardless of how much he talked altogether, how would you rate each member of this group (excluding yourself) on how much he tended to disagree with what others said?
7. (Tension) Regardless of how much he talked altogether, how would you rate each member of this group (excluding yourself) on how much he tended to be nervous, tense, or ill at ease?

The score for each individual on each measure was computed by simply summing the ratings given by the other four members.

The observer's ratings were based directly on his estimates of each person's act proportions for the six Bales-Mann categories *agreement and solidarity* (no. 1), *laughing and joking* (no. 2), *suggestions* (no. 3), *questions* (no. 5), *disagreement and antagonism* (no. 6), and *tension* (no. 7). No reason is given for the observer's not using Bales-Mann category 4, *opinions* and *orientations*. The observer also made ratings on a number of other measures, such as *leadership* and *likability*.

A comparison of the Bales-Mann immediate-recording category definitions and the wording of the questionnaire ratings shows that the category descriptions were simplified for the questionnaire. Where two of the Bales categories were combined, the questionnaire usually used only the higher frequency category as the basis for the questionnaire rating. Thus *agrees* (Bales no. 3) has an average percentage rate of about 9.5, while *shows solidarity* (Bales no. 1) has a percentage rate of only 2.3. In the questionnaire, Mann uses the phrase "tended to agree" as the crucial frame of reference for the Bales-Mann category 1 of *agreement-solidarity*.

Comparing the correlations computed from the immediate-recording percentages and the questionnaire ratings with the semantic-similarity ratings for the Bales-Mann categories required that some choices be made in the wording of the category definitions for a semantic-similarity test. As in the Borgatta, Cottrell, and Mann study, an attempt was made to stay as close as possible to the wording of the immediate-recording categories, but at the same time to follow the technique, used in the questionnaire, of excluding the less frequent Bales category in which combinations of the Bales categories were involved. The phrasing of the Bales-Mann categories for the semantic-similarity test is:

Bales-Mann 1	Agrees with others, complies
Bales-Mann 2	Laughs and jokes
Bales-Mann 3	Gives suggestions
Bales-Mann 4	Gives opinions and states facts
Bales-Mann 5	Asks questions
Bales-Mann 6	Disagrees with others, indicates contrary opinion
Bales-Mann 7	Shows tension and nervousness, withdraws

Originally, a questionnaire similar to that used in the Borgatta study to measure semantic similarity was constructed. However, perhaps because of the increase in the number of categories, and the somewhat smaller but more complex range of meaning contained in the Bales-Mann categories, the rating test proved relatively unreliable. The results showed a systematic but weak correspondence to correlations of the subjects' and observer's questionnaire ratings.

A more reliable and sensitive rating test was constructed by using the method of complete triads. The instructions are given in Figure 3. As can be seen, every triad occurs three times. This makes a longer test than one simply using a scale rating for each pair of terms.

Twenty-eight university undergraduates were tested. The correlation between two randomly split groups of the respondents yielded an r of

This is a test concerning the meaning of words. On each page there will be a word or phrase printed at the top of the page, and then a number of pairs of words or phrases. For example:

Young Man

2. youth	3. boy
3. boy	4. guy
2. youth	4. guy

Circle the word or phrase on each line which is *most similar in meaning* to the word or phrase printed at the top of the page.

Sometimes it will be difficult to choose, but please DO NOT SKIP any of the pairs. Base your judgments on the ordinary, conventional meanings of the words. Work as quickly as you can.

(Ignore the numbers beside each word or phrase. These numbers are for convenience in coding the results.) Now go on the rest of the test.

FIGURE 3.

.94, computed by using mean scores for the number of times each pair of terms was found to be "similar in meaning."

The correlation matrices for immediately recorded behavior and post-session subject and observer ratings, for each of the two experimental group conditions, are presented in Table 3. Table 4 presents the mean semantic-similarity rankings, and Figure 4 the Spearman rank-order correlation coefficients for the comparison of the different matrices.

The results presented in Tables 3 and 4 and Figure 4 reinforce the findings reported from the Borgatta, Cottrell, and Mann study and give additional information concerning the way in which changes in situation affect the different methods. The results may be summarized as follows:

1. As in the Borgatta, Cottrell, and Mann study, the correlations for the immediately recorded behavior percentages are generally smaller than the correlations for the ratings (both the subjects' ratings and the observer's ratings).
2. Even more strikingly than in the Borgatta study, the correlation matrices for the immediately recorded behavior percentages are *not* similar to subject-rating correlation matrices ($r_s = .07$ and $-.03$).
3. Most critically, the correlation matrices for the observer's ratings are *not* strongly similar to the correlation matrices for the immediately recorded behavior ($r_s = .20$ and $.27$).
4. There is a fairly strong degree of similarity between the correlation matrices for the subjects' ratings and the correlation matrices for the observer's ratings ($r_s = .52$ and $.76$).
5. The semantic-similarity rankings of the Bales-Mann categories correspond fairly strongly to the correlations for the subjects' ratings ($r_s = .75$ and $.67$).

TABLE 3. Correlations for Immediate Recordings, Subject Ratings, and Observer Ratings Using Bales-Mann Categories Under Two Experimental Group Conditions.

		Task Conditions						Social-Emotional Conditions					
		Correlations for Behavior Percentages from Immediate Recordings						Correlations for Behavior Percentages from Immediate Recordings					
		2	3	4	5	6	7	2	3	4	5	6	7
Agree	1	.14	−.26	−.25	.17	−.32	−.13	.01	.02	−.44	.01	−.30	−.11
Joke	2		−.27	−.34	−.20	.06	.00		−.37	−.30	−.25	−.18	.02
Suggest	3			−.18	−.23	−.11	−.28			−.33	.37	.25	−.07
Orient	4				−.27	−.16	−.22				−.33	−.33	−.32
Question	5					−.20	.24					.22	−.06
Disagree	6						.06						.14
Tension	7												
		Correlations for Subject Ratings						Correlations for Subject Ratings					
		2	3	4	5	6	7	2	3	4	5	6	7
Agree	1	.16	−.35	−.41	−.18	−.61	.32	.20	.18	−.15	.14	−.55	.04
Joke	2		.31	.34	.28	.16	−.19		.32	.37	.23	.23	−.16
Suggest	3			.83	.53	.49	−.35			.58	.61	.25	−.24
Orient	4				.47	.50	−.46				.39	.43	−.34
Question	5					.45	−.01					.32	−.11
Disagree	6						−.03						.12
Tension	7												
		Correlations for Observer Ratings						Correlations for Observer Ratings					
		2	3	(4)	5	6	7	2	3	(4)	5	6	7
Agree	1	.28	.05		.40	−.34	−.32	.40	.33		.41	−.42	−.48
Joke	2		.34		.16	.05	−.15		.40		.41	−.17	−.33
Suggest	3				.39	.54	−.25				.63	.35	−.24
(Orient)	4												
Question	5					.09	−.26					.07	−.28
Disagree	6						.12						.30
Tension	7												

Source: Mann 1959.

TABLE 4. Mean Semantic-Similarity Rankings (Based on Triads Test).

		1	2	3	4	5	6	7
Agree	1		8.4	5.4	4.9	4.8	0.3	5.0
Joke	2	8.4		4.8	3.6	4.7	2.0	3.2
Suggest	3	5.4	4.8		9.1	7.8	6.0	1.9
Orient	4	4.9	3.6	9.1		6.6	8.2	2.3
Question	5	4.8	4.7	7.8	6.6		6.6	4.0
Disagree	6	0.3	2.0	6.0	8.2	6.6		5.1
Tension	7	5.0	3.2	1.9	2.3	4.0	5.1	

NOTE: The greater the mean figure, the higher the similarity ranking.

6. The semantic-similarity rankings of the Bales-Mann categories correspond strongly to the correlations for the observer's ratings ($r_s = .61$ and $.91$).
7. The semantic-similarity rankings of the Bales-Mann categories do

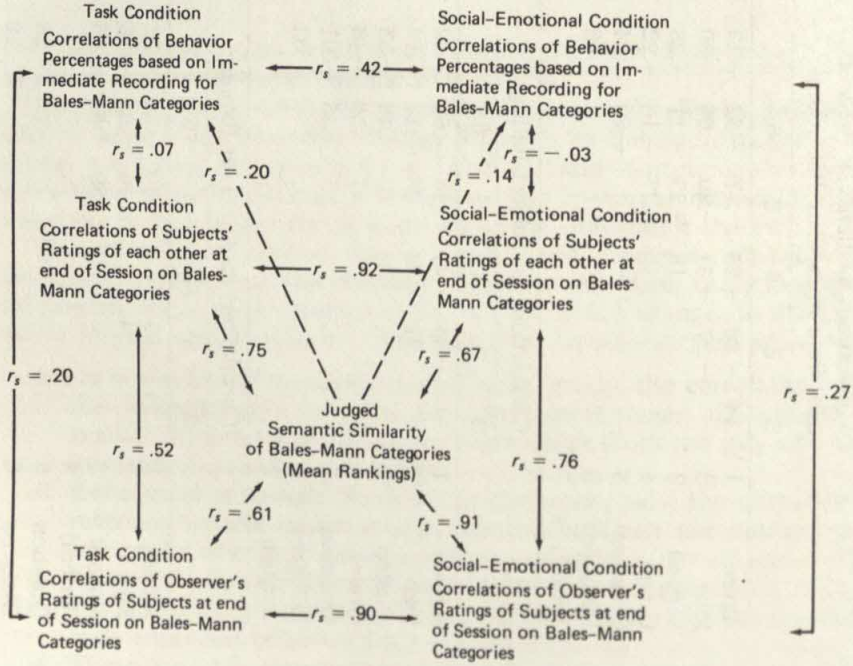

FIGURE 4. Comparison of correlation matrices for behavior percentages based on immediate recording, postsession subject ratings, postsession observer ratings, each under two group-experimental conditions, with semantic-similarity rankings, all using Bales-Mann category system.

not correspond strongly to the correlations for immediately recorded behavior percentages ($r_s = .20$ and .14).

8. The correlation matrices for the observer and subject ratings are very similar across experimental conditions ($r_s = .92$ for subject ratings, and .90 for observer ratings). The immediately recorded behavior percentages, on the other hand, do not show a high degree of cross-situation stability ($r_s = .42$).

The pattern of results shows a consistent network of congruence between the semantic-similarity rankings and the correlation matrices for ratings made on the basis of long-term memory, regardless of whether the ratings were made by subjects or by the observer, or under group task conditions or group social-emotional conditions. In contrast, the correlation matrices for the immediately recorded behavior percentages are *not* strongly similar to any kinds of ratings made on the basis of long-term memory, and they show considerable change across group conditions.

The hetromethod correlation matrices for immediately recorded behavior percentages, subject ratings, and observer ratings are presented in Table 5.

The correlations in Table 5 show only a slightly more consistent pattern than the hetromethod matrix from the Borgatta, Cottrell, and Mann study (Fig. 4). Generally, the categories for the immediately recorded behavior percentages have their highest correlations with the properly corresponding categories for subject and observer ratings (underlined coefficients). These *validity* coefficients for the immediately recorded behavior percentages and the observer ratings are slightly higher than the validity coefficients for the immediately recorded behavior percentages with the subject ratings. However, the median validity correlation coefficient, which is between .33 and .36, is still unacceptably small. These data indicate that there has been considerable slippage in the recall of events.

GENERAL FORMULATION

The argument presented in this chapter is not just that there is memory drift when people make ratings or rankings of other people's behavior, but that this 'drift' is systematic, nonrandom, biased in the direction of the rater's conception of "what is like what." More abstractly stated, it is hypothesized here that, given a series of attributes (such as behavior traits) which can apply to a class of objects (such as other people), there will be systematic shift in the individual's recall of which attributes are possessed by which objects, such that the more strongly the individuals conceive of the attributes as belonging together, the more likely it will be that the individual recalls both attributes as belonging to the same objects. With this type of memory error, any attempt to discover how human behavior is organized into multibehavior units—such as dimensions or clusters—which is based on data consisting of long-term memory judgments will result in conclusions which primarily reflect the cognitive structure of the raters.

TABLE 5. Hetro Method Correlations for Bales-Mann Categories under Two Experimental Group Conditions.

Subject Rating \ Immediately Recorded %	Task Condition 1	2	3	4	5	6	7	Social-Emotional Condition 1	2	3	4	5	6	7
1	+.41	+.29	−.44	−.21	+.31	−.30	+.30	+.36	+.27	−.14	+.10	−.08	−.53	−.24
2	+.07	+.37	−.17	+.16	−.10	+.10	−.13	.00	+.25	−.10	+.09	+.04	−.00	−.44
3	+.01	−.23	+.20	+.42	−.14	+.03	−.39	+.19	−.23	+.20	+.12	+.30	−.16	−.45
4	+.06	−.17	+.20	+.54	−.16	−.01	−.37	−.02	−.26	+.05	+.23	+.07	+.06	−.49
5	+.04	−.20	+.03	+.10	+.10	+.03	−.21	+.11	−.18	+.04	+.08	+.36	−.09	−.28
6	−.30	−.26	+.07	+.29	−.06	+.32	−.15	−.31	−.31	+.04	+.10	+.15	+.53	−.07
7	+.19	+.16	−.22	−.07	+.59	+.13	+.55	−.16	+.15	−.13	+.06	−.10	−.02	+.14
Observer Rating \ Immediately Recorded %	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**1**	**2**	**3**	**4**	**5**	**6**	**7**
1	+.34	−.01	−.05	(.08)	+.14	−.49	−.29	+.30	+.09	−.05	+.08	+.01	−.41	−.46
2	−.00	+.24	−.06	(.09)	+.09	−.10	−.29	+.25	+.33	−.06	+.08	+.12	−.10	−.30
3	−.19	−.30	+.29	(.30)	−.14	+.02	−.37	+.12	−.21	+.33	+.18	+.30	+.12	−.47

Subject Rating \ Immediately Recorded %	Task Condition 1	2	3	4	5	6	7	Social-Emotional Condition 1	2	3	4	5	6	7
5	+.14	−.32	+.05	(.19)	+.18	−.29	−.31	+.20	−.02	+.18	−.07	+.48	−.01	−.36
6	−.13	−.19	−.02	(.24)	−.10	+.51	−.06	−.18	−.27	+.20	−.20	.18	+.65	+.07
7	−.20	.00	−.38	(.00)	+.06	+.22	.59	−.21	+.04	+.12	−.17	−.05	+.35	+.55

Observer Rating \ Subject Rating	1	2	3	4	5	6	7	1	2	3	4	5	6	7
1	+.26	+.18	+.19	(+.17)	+.30	−.16	−.09	+.24	+.16	+.26	(+.28)	+.21	−.16	−.22
2	+.11	+.52	+.25	(+.32)	+.12	+.02	−.32	−.18	+.33	+.28	(+.24)	+.15	−.09	−.25
3	−.40	+.28	+.64	(+.68)	+.28	+.39	−.42	−.13	+.34	+.61	(+.48)	+.33	+.23	−.35
5	−.12	+.26	+.46	(+.42)	+.34	+.16	−.16	+.02	+.15	+.48	(+.32)	+.39	+.05	−.34
6	−.37	+.10	+.41	(+.38)	+.10	+.41	−.16	−.47	+.04	+.06	(+.15)	+.01	+.51	−.02
7	+.08	−.23	−.22	(−.27)	−.13	−.01	.25	−.29	−.13	−.22	(−.19)	−.21	+.17	+.20

Source: Mann 1959, Appendix G.
Note: 1 = agrees; 2 = jokes; 3 = suggests; 4 = orients; 5 = questions; 6 = disagrees; 7 = tension.

REDISCOVERY

After completing a first draft of this chapter, I discovered a reference in an older textbook on psychometric methods (Guilford 1936) to a study done in 1931 which clearly states the major hypotheses presented here, supported by differences in correlations found between observer ratings based on long-term memory and the correlations found between immediately recorded behavior frequencies (Newcomb 1931). The subjects were thirty problem boys sent to a summer camp for five weeks. The boys were under the constant observation of a psychiatrist and six or more trained counselors. A daily record was kept of specific incidents involving twenty-six categories of behavior for each boy by his own counselor. Some 8,500 incidents involving these categories were also recorded by the experimenter. At the close of the camp period ratings were obtained from each of the seven observers on the frequency of these twenty-six categories of behavior for every boy.

A mean correlation of .41 was found for the relation between daily record scores and the memory based ratings, indicating a weak degree of correspondence between methods. The correlations between behavior categories, however, showed a considerable degree of distortion when the results from the two different methods were compared. Intercorrelation of the categories for nine general traits yielded a mean correlation figure for the memory-based ratings of .49, but a figure of only .14 when computed from the daily records. Concerning these results Newcomb states: "The conclusion may therefore be drawn that the halo effect, inevitable in the ratings, *worked in such a way as to cause the rater to rate similarly logically related behaviors*. . . . The close relation between the intra-trait behaviors which is evident in the ratings may, therefore, be presumed to spring from logical presuppositions in the minds of the raters, rather than from actual behaviors" (Newcomb 1931, p. 288; italics added).

SIMILARITY

A critical problem in the formulations presented above concerns the lack of specificity in some of the terms, and especially the vagueness of the term "similarity" when used with reference to an individual's conceptions. In this chapter measures of conceptual similarity have been phrased primarily in terms of semantic similarity. Thus the instructions for the questionnaire and the ranking task stressed that the similarity judgments should be based on the degree to which the events named by the terms were actually alike, rather than the degree to which the terms elicited the same associations.

In my earlier paper on trait psychology and componential analysis the theoretical discussion emphasized *denotative similarity;* that is, the degree to which different terms shared the same *distinctive features,* or *criterial attributes* (D'Andrade 1965).

Shweder (1972), in a study which demonstrates that the structural organization of variables obtained by Bales (1969) in his work with small

groups can also be obtained by having respondents sort the relevant variables on the basis of "similarity," has pointed out that any assumption that the respondents are actually using overlap in distinctive features as the primary basis for making similarity judgments is unwarranted. Shweder argues that the basis on which the respondents make similarity judgments of this type is the degree to which attributes contiguously go together in making up a symbolic *behavioral type,* which he treats as a learned cultural construct.

Work by other investigators also indicates that a number of different kinds of relationships may be involved in judgments of similarity. Flavell and Flavell (1959) and Flavell and Stedman (1961) have presented evidence that judgments about similarity in meaning are affected by the *logico-grammatical relationships* occurring between terms. In the Flavell and Stedman study children and adults judged which of two pairs of terms were more similar in meaning (e.g., *big-large* versus *throw-ball*). Across a large number of such judgments, a relatively stable and consistent rank order in similarity was found for the various categories of logico-grammatical relationships by approximately age ten. Highest in the rank order of similarity were *synonymous* pairs (big-large, steal-rob), then *similar-dimension* pairs (small-tiny, smile-laugh), then *superset-subset* pairs (bird-sparrow, tree-oak), followed closely by *whole-part* pairs (bird-wing, shoe-heel), *object-attribute* pairs (lemon-sour, mouse-small) and *common-action-of* pairs (dog-bark, lion-roar).

Further down the scale were *coordinate* pairs (cow-horse, pipe-cigar), *common-action-upon* pairs (sweep-floor, chew-gum), *common-use* pairs (dog-bone, farmer-tractor), and *part-part* pairs (pedal-handlebars, wall-floor). Last were *contrast-on-a-dimension* pairs (hard-easy, strong-weak).

Results such as the Flavell and Stedman rank ordering could not be due to judgments based solely on overlap in distinctive features. Many other kinds of criteria appear to be involved in similarity judgments. In any case, it is somewhat ironic that the problem of not knowing what is happening when people are used as measuring instruments, raised at the beginning of this chapter and given as the reason why certain procedures in the social sciences produce invalid results, returns to plague a later formulation about exactly how these invalid results come about.

WIDER IMPLICATIONS AND ALTERNATIVE SCHEMAS

If the argument presented here concerning the lack of validity of procedures for discovering how behavior is organized when these procedures are based on correlations of long-term memory judgments is correct, a large number of studies in the social sciences are brought into question. Correlational studies relying on memory-based checklists, ratings, or interviews are obviously placed in the "dubious" category (Burton 1970). Most of these correlational studies have attempted to show that certain general traits or dimensions of behavior make possible an economic description of personality, or child rearing, or interpersonal behavior. But if the correlations on which these studies rest are primarily an artifact of the rater's or the questionnaire taker's cognitive structure, and not a

reflection of the real world, there is little or no evidence that human behavior can be described by large multibehavior units. What remains is a world in which human behavior is to be described in terms of specific behaviors occurring in specific situations, as Mischell and others have argued (Mischell 1968).

A world made up of numerous ungroupable behaviors might seem disadvantageous for attempts to describe human behavior. Without multibehavior unit constructs, such as extroversion, assertiveness, intelligence, and so forth, it might seem that the goal of accounting for a large portion of an individual's behavior with a relatively small number of descriptive terms is not feasible. There is an alternative to the multibehavior unit schemata, however, one based on the analysis of the distribution of frequencies with which specific behaviors are performed.

Frequency distributions for category systems applicable to human behavior display very common forms or shapes, and these shapes make possible brief descriptions which account for large portions of behaviors. The shape of a frequency distribution for the nominal or unordered classes typical of most behavior-category systems is usually drawn as a rank-frequency graph. On this graph the categories are arranged in rank order of frequency along the horizontal axis, with the most frequent category next to the origin and with the actual frequencies (or proportions of the sum total of all categories) scaled along the vertical axis. Obviously, for any set of frequencies, a line through the frequency plots for each rank will decrease monotonically (i.e., each point will be lower than the point to its left). Figure 5 presents an example of a rank-frequency graph, with four possible kinds of rank-frequency relations plotted by the curves *A*, *B*, *C*, and *D*.

One of the most general findings in the social sciences is that rank-frequency plots for a variety of classifications of behavior have shapes like curve *D*, which shows a rapid drop from the first- to the second-ranked category, and successively smaller drops as rank decreases. In a number of cases these concave-shaped frequency relations, when graphed on log-log coordinates, display straight lines. Such curves are often called Zipf curves. For the argument here, however, the curve does not have to be a true Zipf curve, but it must have a pronounced concave shape.

Figures 6 and 7 present rank-frequency plots for the proportional-frequency data from the two experimental group conditions of Mann's 1959 study. Both plots show pronounced concave forms, with the social-emotional condition displaying an even steeper initial drop than the task condition. There are other interesting differences between the two group conditions; *suggests* and *disagrees* are lower in rank order for the social-emotional condition than for the task condition, while *jokes* is higher.

The steep initial drops from the high-ranking categories show that most of the behavior of these small groups occurs in just a few categories. In the task condition group the two categories of *opinions* and *suggestions* account for 65 percent of all the behavior. In the social-emotional condition group the two categories of *opinions* and *agreement* account for about 70 percent of all the behavior.

A concave shape of a rank-frequency curve indicates that a good predic-

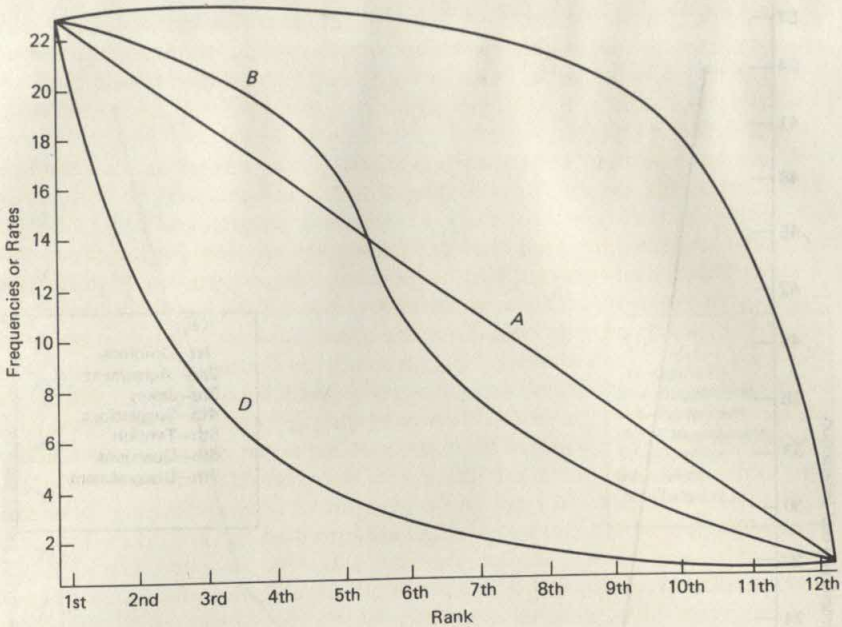

FIGURE 5. Rank-frequency graph.

tion of what the group or individual is doing can be gained simply by guessing that the group or individual is performing a behavior belonging to a high-rank category. Or, to put it another way, the concave curve indicates that the group or individual mainly performs its most frequent behaviors. It should be stressed that, if the rank-frequency plot does not have a concave form, one cannot obtain a good prediction by assuming that an individual is performing his most frequent behaviors. If the shape of the curve is convex, for example, like curve *C* in Figure 5, there is little improvement in prediction in saying that the individual is performing his most frequent behavior rather than his next-to-most frequent behavior.

The concave form of rank-frequency relations implies that it is not necessary to group behaviors into clusters, or traits, or dimensions to be able to give an economical description of an individual's behaviors. Instead, by using a number of different category systems and plotting the rank-frequency relations for each one, a good description of a person's behavior can be given by "skimming off" the top-ranking categories from each classification system and then describing that person as highly likely to be performing these few behaviors. Thus, if one categorizes a person's gestures, his paralinguistic repertoire, the durations of his speech and silence, his choice of conversational topics, his selection of behavior settings, and so forth, and simply picks the highest frequency category for each of these classifications, a remarkably predictive description can be obtained.

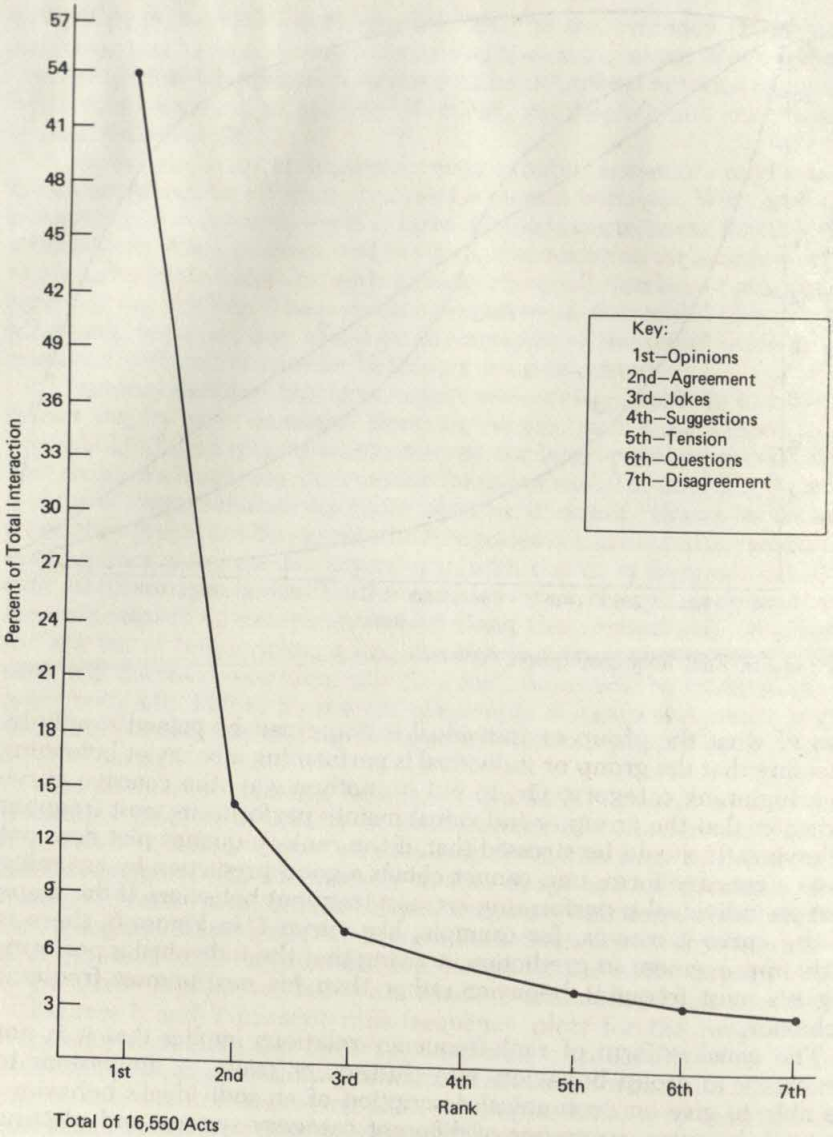

FIGURE 6. Bales-Mann categories for task-oriented group.

The usefulness of describing individuals or groups in terms of their high-frequency behaviors depends on the degree of concavity of the rank-frequency relation. But why does the rank-frequency relation almost always show a concave shape for behavior-category systems? There seem to be various possible causes. Zipf curves, for example, can be generated by a random-walk process (Miller and Newman 1958); and other random

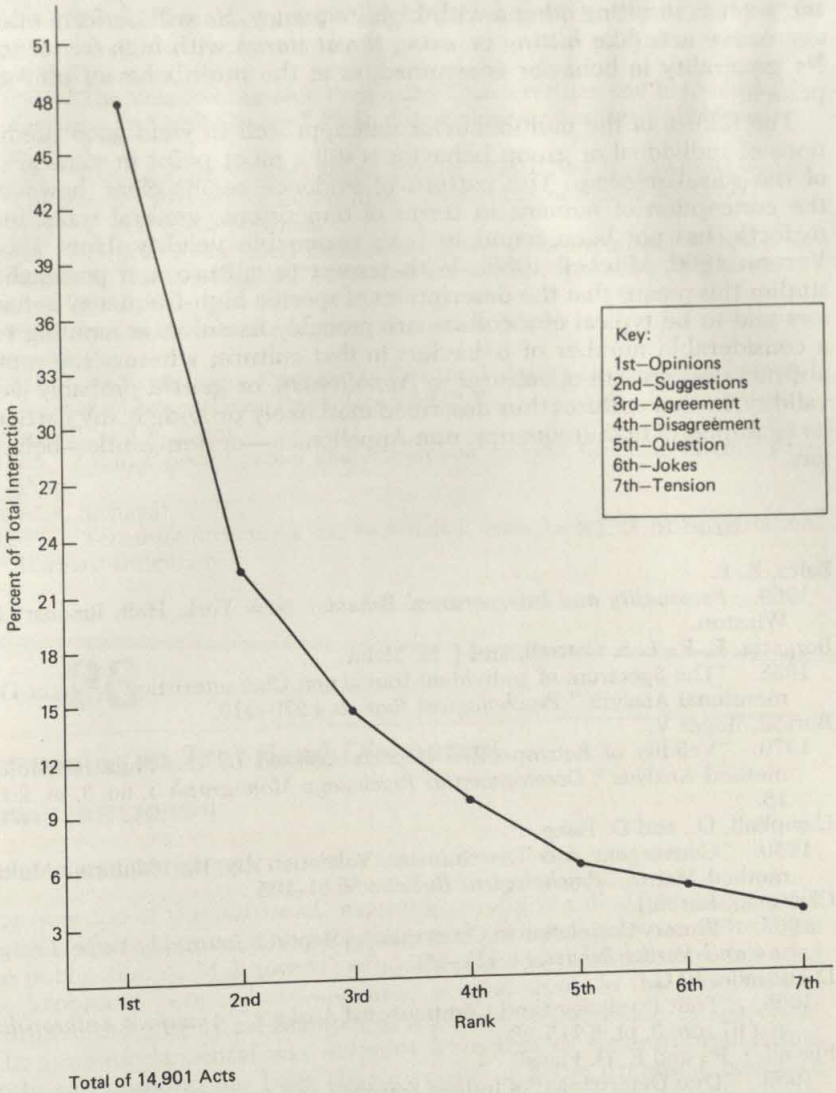

FIGURE 7. Bales-Mann categories for social-emotional group.

processes, such as binomial series, typically show concave curves when plotted in rank-frequency charts. Thus the concavity of the rank-frequency relation may simply be a result of the nonorganized features of behavior determined by our descriptive methods.

The critical difference between this behavior-specific approach to description and the multibehavior unit approach is that the former does *not* assume that, because an individual performs one specific aggressive

act (such as insulting others) with high frequency, he will perform other aggressive acts (like *hitting* or *using threat stares*) with high frequency. No generality in behavior is assumed, as in the multibehavior unit approach.

The failure of the multibehavior unit approach to yield good predictions of individual or group behavior is still a moot point in some areas of the social sciences. The pattern of evidence seems clear, however: the conception of humans in terms of dimensions, general traits, and so forth, has not been found to have reasonable validity (Hunt 1965; Vernon 1965; Mischell 1968). With respect to culture and personality studies this means that the descriptions of specific high-frequency behaviors said to be typical of a culture are probably useful in accounting for a considerable number of behaviors in that culture; whereas the more abstract descriptions of cultures as *Appollonian*, or *gentle*, probably lack validity, in that cultures thus described most likely provide many particular examples of high-frequency, non-Appollonian—or non-gentle—behaviors.

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23

Reasoning in Trobriand Discourse[1]

EDWIN HUTCHINS*†

INTRODUCTION

The question of the nature of reasoning among so-called 'primitive' peoples has a long history in anthropology and philosophy. In the years since the publication of Malinowski's ethnographies of the Trobriand Islands,[2] the Trobriand people have appeared several times in the literature of primitive thought as an example of a people whose mode of reasoning is in some fundamental way different from that of Western civilization. Trobriand thinking has been characterized as being irrational, concrete rather than abstract, and dominated by right hemisphere (affective/integrative) rather than left hemisphere (rational/analytic) processes.

The strongest and most detailed claims have been made by D. D. Lee (1940, 1949). Working exclusively from Malinowski's published materials, Lee asserts that, "To the Trobriander, events do not fall of themselves into a pattern of causal relationships, as they do for us" (1949:406).

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† Source: *The Quarterly Newsletter of the Laboratory of Comparative Human Cognition,* 1:2, pp. 13–17, 1979.

She contends that the language of the Trobriands lacks terms which posit relations among propositions and claims,

> This does not mean that the Trobrianders are incapable of explaining a sequence in terms of cause and effect, but rather that this relationship is of no significance. (1949:407)

Lee's denial of the concept of causal relations leads her to assert, with regard to motivation, that the Trobrianders not only do not interpret acts in terms of intents, but that they do not in fact even have intents. "The Trobriander performs an act because of the act itself, not for its effects" (1949:408). Lee concludes her 1949 paper as follows:

> Whether they are given or read into reality by us, temporality, causation, teleology, and relationship in general have neither meaning nor relevance for Trobriand behavior. (1949:415)

Such claims may seem credible when evaluated only in the light of the limited data Lee presents, and, in fact, many anthropologists are impressed by her arguments. For one who has lived in the Trobriands, learned the language, and experienced the complexity of everyday life there, however, these claims are absurd. In this paper I will present an analysis of an example of natural Trobriand discourse. From this analysis, I will draw what I think are some more reasonable conclusions about the nature of Trobriand reasoning. In addition, I will provide an explanation of the source of Lee's mistaken conclusions and consider some issues concerning the conditions under which we are likely to be justified in making inferences about other people's thinking.

The analysis presented here will focus on deduction, the sort of inference which involves the assignment of a degree of likelihood to one proposition on the basis of the likelihoods of other propositions. In our society, the rules of deduction in symbolic logic are exemplary inference forms. In classical symbolic logic, only two degrees of likelihood are considered; a proposition is either true or else it is false. The rules of deduction in this system are a special case of inference as defined above. While they are widely taken as a normative standard, they are not in fact accurate descriptions of how most of us make inferences most of the time. We often think in terms of a continuum of likelihoods, and we make plausible inferences where strong inferences are not available.[3] That is, rather than infer that something is strictly true or false on the basis of some observation, we may instead only be able to infer that it is likely or unlikely. I am making no claims here about the nature of the mechanism by which the adjustment of likelihoods is accomplished. Whether it is Bayesian or otherwise is a topic for another paper. What I am arguing is that even in naturalistic settings, we can observe adjustments of likelihoods, and assess the direction (up or down)—if not the amount—of change in the likelihood of a proposition. A rough model of inference then, contains these elements: (1) a proposition which relates two or more concepts (e.g. P implies Q), (2) a new bit of information, either retrieved from memory or gleaned from the interpretation of the world of experi-

ence, concerning the likelihood of one of the concepts related by the first proposition (e.g. P is very likely), and (3) a procedure for determining what impact the new information has on the other concept related in the first proposition (e.g. Q is also very likely).

Some sort of inference must be utilized any time one makes an assertion, or attempts to support an assertion, about the likelihood of an event or state that has not been directly observed. This means that inference is involved in virtually every instance of planning. Much of the apparent richness of our experience derives from our use of inference to fill in and elaborate our representations of the world.[4] Sometimes we use inference to establish the likelihood of events which are technically observable, but which have not actually been observed. Other times inference tells us about events, such as the intents and desires of other social actors, that we can never observe directly.[5]

That people make inferences is easily demonstrated by pointing to those things we all do that require inference. Specifying just where, how, and on the basis of what information people make inferences is, however, somewhat more difficult. A major problem in the study of inference in natural discourse is that much of discourse is composed of syllogisms in which one or more premises are left unstated. If I say to you, "Socrates is mortal because he is a man," you know what I mean. But it is not necessary for the purposes of communication to explicitly state the missing major premise—vis., that all men are mortal. That premise is implicit in your understanding of what I have said even though it does not appear in the discourse. Given a community of speakers with some shared set of understandings about how the world works, it would be both redundant and tedious for each of them to exhaustively specify all of the premises used in his reasoning when he is communicating that reasoning to his fellows.

The task of identifying inferences in natural discourse, then, must involve a specification of the premises on which the inferences are based. In the following section I will present a brief excerpt of discourse from an important area of Trobriand life. I will also attempt to demonstrate that an ethnographically informed model of the cultural knowledge about the events in this domain can provide the premises missing from the discourse itself, thus permitting us to analyze the inferences that are being made.

CULTURAL PREMISES

Consider an example from the domain of Trobriand land litigation. The cultural premises that underlie reasoning about claims to land concern the conditions under which rights in land can be transferred from one person to another. The full set of premises for this domain is much too large and complex to present here. A small subset of those premises, however, accounts for a large fraction of the inferences made in litigation. A simplified account of that subset will be presented here.

There are two necessary, but not sufficient, conditions for a legitimate

transfer of rights in land. The first is simply that the person who is giving rights in land must, at the time of the transfer, have rights to give. That is, this person must have previously and legitimately acquired the rights, and must not yet have transferred the rights to someone else. When this condition is met, the land is said to be TUPWA, "still remaining" with respect to that person. Once a person has transferred his or her rights in land to someone else, that land is no longer TUPWA; it is said to be KASESILA, "decided upon" or "allocated" with respect to that person. The satisfaction of this first condition is summarized in Trobriand discourse by the term TUPWA.

The second condition is that the person who is to acquire rights must have provided the rights holder with an instance of at least one of several classes of exchange inducements called POKALA. Regardless of the form taken, POKALA as an inducement is given with the hope, but without the guarantee, of a reciprocal presentation. This second condition for legitimate transfer is summarized in discourse by the term POKALA or by any of the more specific terms for its sub-categories.

A basic premise, then, is that a legitimate transfer of rights in land implies both that at the time of transfer the land be TUPWA with respect to the person giving the rights, and that the aspiring rights holder have provided POKALA to the person giving the rights. A second premise is that whenever a legitimate transfer has taken place, the recipient of the rights in land does in fact have those rights. This one seems so trivial as to not deserve mention, yet it is an important premise upon which many inferences are based.

A LAND DISPUTE

In July of 1976, two men disputed each other's claims to rights in a particular piece of garden land. A village court was convened to hear the case. Each man made a presentation to the court in which he gave an account of the sequence of events he believed led to his having rights in the land. The two accounts began with the same historical events, but they diverged with respect to a transfer of rights about thirty years in the past. Both litigants described a POKALA presentation by a third man to a woman who previously held rights in the garden (see Figure 1, t1). The first litigant to make his presentation to the court maintained that this POKALA attempt was unsuccessful; that it was not responded to with a transfer of rights. He claimed that the garden remained TUPWA at this point (t2), and that the woman later gave the garden to him in response to his subsequent POKALA (t3). The second litigant maintained that the rights to the garden were transferred to that third man in response to his POKALA (t3), making the garden KASESILA with respect to the old woman. This litigant claimed that he later acquired rights to the garden, not from the woman, but from that third man who had provided the earlier POKALA (t2-t4).

As stated, the claims of both litigants are plausible. They each describe a possible history of the garden which conforms to the principles of land tenure. In the course of the testimony of witnesses it became clear that

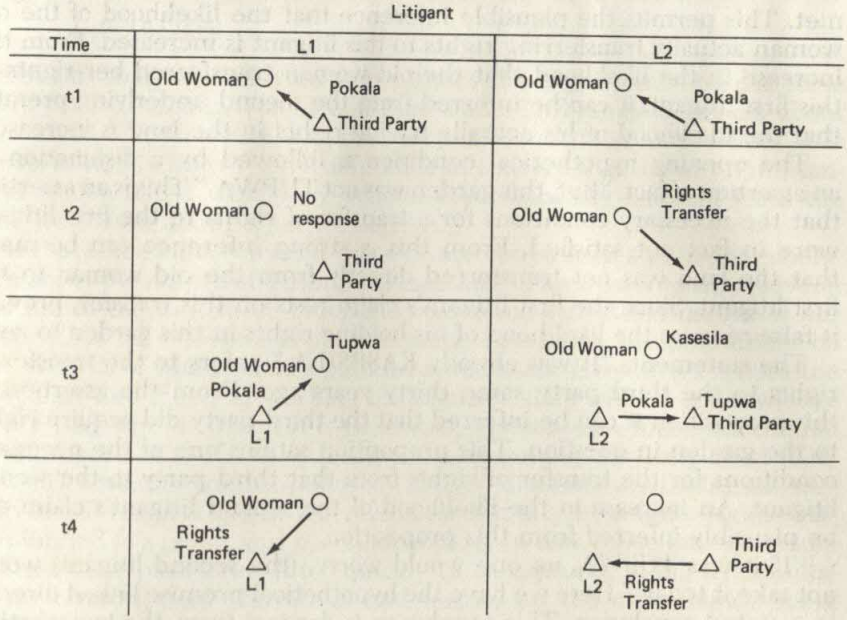

FIGURE 1.

the POKALA attempt by the third man some thirty years ago was indeed successful and was followed by the transfer of rights in the garden to him.

The following is a translation of an excerpt of the court decision directed to the losing litigant:

> With regard to this garden: Well, within the argument, if it had been TUPWA, you all understand. You all know how it is with POKALA and TUPWA. But this garden was not TUPWA. It was already KASESILA, and this is the source of it. . . . My hearing of it, my unbiased opinion is this: If it had been TUPWA, no one would worry, [the second litigant] would not take it today, it would be your thing. But because it was previously decided upon, fine, I say, let it be KASESILA.[6]

ANALYSIS

The decision begins with a hypothetical condition, "If the garden was TUPWA," and explicitly invites the listeners to make the appropriate inference, "You all know how it is with POKALA and TUPWA." What they all know is that POKALA and TUPWA together are the necessary conditions for a transfer of rights. This is, of course, the major underlying premise. The first (losing) litigant's POKALA to the old woman is not questioned, so that if it were true (at time t3) that the garden was TUPWA, then the necessary conditions for a transfer of rights would have been

met. This permits the plausible inference that the likelihood of the old woman actually transferring rights to this litigant is increased. From the increase in the likelihood that the old woman transferred her rights to this first litigant, it can be inferred from the second underlying premise that the likelihood of his actually having rights in the land is increased.

The opening hypothetical condition is followed by a disjunction to an assertion of fact. "But, this garden was not TUPWA." This is an assertion that the necessary conditions for a transfer of rights to the first litigant were in fact not satisfied. From this, a strong inference can be made that the land was not transferred directly from the old woman to the first litigant. Since the first litigant's claim rests on this transfer, proving it false reduces the likelihood of his holding rights in this garden to zero.

The statement, "It was already KASESILA," refers to the transfer of rights to the third party some thirty years ago. From the assertion of this proposition it can be inferred that the third party did acquire rights to the garden in question. This proposition satifies one of the necessary conditions for the transfer of rights from that third party to the second litigant. An increase in the likelihood of the second litigant's claim can be plausibly inferred from this proposition.

"If it was TUPWA, no one would worry, (the second litigant) would not take it today." Here we have the hypothetical premise linked directly to a stated conclusion. This conclusion is derived from the hypothetical premise and the implicit premise as follows: from the hypothetical proposition that the garden was TUPWA at the crucial point in time (t3), it can be inferred that the third party did not acquire rights in the garden. That allows the strong inference that the third party could never have transferred his rights to the second litigant. That inference would clearly destroy the second litigant's argument.

"If it had been TUPWA, it would be your thing." This is yet another inference from the counterfactual hypothetical proposition which lies at the center of the first litigant's argument. The inference structure here is precisely as it was in the first statement of the decision. It provides for the plausible inference of the first litigant's claim being true.

Finally there is one more disjunction from the hypothetical to the factual. As shown earlier, the assertion of the garden status KASESILA allows inferences which show the second litigant's claims to be plausible and the first litigant's claims to be impossible.

Understanding this brief fragment of discourse requires a total of twelve inferences. Six are weak plausible inferences, and six are strong deductive inferences. All of the inferences are based on the simplified major premises of land tenure defined earlier in terms of causal and temporal relations among abstract classes of events. The act of either understanding or producing this bit of discourse requires 1) the ability to treat concrete instances as members of abstract event classes, 2) a comprehension of the nature of the causal and temporal relations between abstract event classes, and 3) the ability to determine the truth values of hypothetical concepts in accordance with their logical relations to other concepts whose truth values have already been established.

DISCUSSION

From this analysis, and from many others like it performed on discourse from other domains, I conclude that while Trobrianders' beliefs about the world are, in some domains, very different from our beliefs, it is unwarranted to infer from a difference in content that the way Trobrianders reason about what they believe is substantially different from the way we reason about what we believe.

If that is the case, why is it that Lee arrived at such a different conclusion about the nature of Trobriand reasoning? The answer to this question lies not in the nature of Trobriand reasoning, but in the nature of our procedures for knowing about reasoning. Lee based her conclusions on her analysis of Malinowski's published materials. Even if we ignore the problems Lee faced in working on linguistic data without a knowledge of the language in which it is expressed, Malinowski's materials contain some important artifacts with respect to reasoning. Virtually all of Malinowski's verbatim transcriptions of "native" discourse are traditional narratives and magical spells. The reason for this is quite simple. Malinowski was limited to a paper and pencil technology in data collection. Spontaneous discourse passes much too quickly to be captured in full by simultaneous transcription. Narratives and magic, on the other hand, being memorized formulas, can be recited for the note-taking ethnographer at a pace with which he or she can cope. But narratives and magic are generally poor in inference. The comprehension of narrative discourse requires inferences, but the inferences are not themselves made explicit in the discourse. Magical spells, it turns out, are like telegraphic speech, so devoid of overt specification of relations that they often seem cryptic even to Trobrianders. Litigation, and other speech contexts which require explicit explanations of the relations among events, are much richer in inference than these other domains. I cannot help but think that some opinions about Trobriand thought are in part an artifact of the preponderance of magic and narrative and the dearth of natural conversation in Malinowski's published texts. Malinowski is, of course, not to blame. Natural conversation is virtually impossible to capture without the help of a tape recorder. It strikes me as a bit ironic, however, that our opinion of their intellectual abilities may have been in some way dependent upon the state of our own technology.

Goody (1977) has argued that the development of logic:

> seemed to be a function of writing, since it was the setting down of speech that enabled man to clearly separate words, to manipulate their order, and to develop syllogistic forms of reasoning; . . . (1977:11)

Goody is right that it is the setting down of speech that enables man to separate words and manipulate their order. The notion of syllogistic reasoning as a thing to be described is a meta-linguistic concept, and language must be objectified for the development of meta-linguistic concepts. This is the key to Lee's problem. In analyzing Malinowski's material

Lee was unable to find syllogistic reasoning for at least two reasons. First, Malinowski was unable to set down a written record of the sorts of speech in which syllogistic reasoning is made explicit, and second, having never lived in the Trobriands, Lee had no access to, and thus could not make explicit, the implicit premises underlying the discourse which was recorded.

But if Goody means that the ability to do syllogistic reasoning depends on writing, then he is also wrong, because he has confused the technology required of the analyst for a *description* of reasoning (setting down a written record) with the technology required for the *performance* of reasoning. Just as writing is required for a description of the syntax of a language, but not for the performance of grammatical speech, writing is necessary only for the description, not for the production of syllogistic reasoning.

The failure of non-literate peoples to solve experimenter-posed syllogistic tasks is not indicative of an inability to do syllogistic reasoning, but of the failure to conceive of syllogistic reasoning meta-linguistically as a "structure" of reasoning which can be applied in abstraction to novel as well as familiar situations. In this paper I have argued, I hope successfully, that when reasoning in a domain which is structured by a set of meaningful cultural premises, non-literate peoples do employ syllogistic forms of reasoning which are formally indistinguishable from the everyday reasoning of Western man.

NOTES

1. The research reported here was conducted by the author in Milne Bay Province of Papua New Guinea in 1975 and 1976 under a dissertation research grant from the Social Science Research Council. The analysis in this paper is taken from my doctoral dissertation, Hutchins (1978). My thanks to Michael Cole for his criticism and advice on this version of the material.
2. Malinowski published seven books and several articles on the Trobriands between 1922 and 1935. The most important of these for this paper are (1922, 1929, 1935a, 1935b).
3. See Polya (1954) and Collins and Larkin (1977) for two views of plausible inference.
4. The recent literature of discourse comprehension is rich in demonstrations of the importance of inference to fill out interpretations (cf. Bobrow & Norman, 1975; Schank & Abelson, 1977).
5. cf. Hutchins 1974.
6. The text reported is a translation by the author of a case excerpt which was originally recorded on stereo tape. The transcription prior to translation was made by the author and checked with informants. The relational terms are given the same translations as those used by Malinowski (contra Lee).

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